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CONTENTS.

ARTICLES. PAGE

1. The Velocity of Light. By O. REICHENBACH 505
2. The Poisoning Cases of the last Half-Century 519
3. Benjamin Franklin and the Origin of Lightning Rods. By Col. A. PARNELL, R.A... 527
4. Heath's Bolivian Explorations. By Rev. J. D. PARKER .. 536
5. On Technical Education. By ROBT. GALLOWAY, M.R.I.A. 541

ANALYSES OF BOOKS.

Wieder die Humanasters! Rechtfertigung eines Vivisectors .. 552
Micro-Photography, &c. 554

ANALYSES OF BOOKS—*continued.* PAGE

Elementary Text-Book of Physics 555
Household Chemistry; for the Non-Chemical 556
Economy of Coal in House Fires 557
Report of an Exploration of Parts of Wyoming, Idaho, &c... .. 558
Second Report of the Department of Entomology of the Cornell University Experiment Station 558
Memoirs of the Geological Survey of India 559
Breeding Horses for Use 560
Remarks on Cattle-Plague Vaccination 561

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THE

JOURNAL OF SCIENCE.

SEPTEMBER, 1883.

I. THE VELOCITY OF LIGHT.*

By O. REICHENBACH.

"Hence the agreement or disagreement of the values of V and v furnishes a test of the electro-magnetic theory of Light."—CLERK MAXWELL.

ALL matter† being equally subject to gravity, the variety in its distribution leading to variety of motions can be the only cause of variety of matter.

Laplace's $V = \sqrt{gHK \div D}$ gives for air, at 15·22 C., the mean temperature at the surface of the earth, under 760 m.m. pressure; the velocity of sound 345 metres in a second. The atmosphere, however, contains water-stuff in every form, which wind raising accelerates and retards sound.

The water-stuff always in the air, o. s. p., is 347 centims. over the earth, the $1 \div 29{\cdot}78$ of the mass of the atmosphere. This water propagating sound with a velocity to that in air as the respective specific heats 4·25 : 1, the mean increase of the velocity of sound through the whole of the atmosphere is 51 metres:—

$$345 \text{ m.} + 51 \text{ m.} = 396 \text{ metres,}$$

which is the mean rotary velocity of the surface of the earth, o. s. p.

* Referring frequently to "On some Properties of the Earth" (Wertheimer and Lea: London, 1880) I designate it by o. s. p.

† In 1849 I wrote, not unprophetically, "Without matter no sound. Sound deranges the parallelism of the light orbits of atoms. Sound gives an image. Sound displaces light; one might possibly observe oscillations of the dark rays of light by a strong sound; the more elastic the medium through which the light moves, and the denser the medium on which it is received, the better. With patience one might magnetise iron by sound."

This solar aqueous disturbance is regulated in its more and less by the 7·78 times the 347 centimetres are annually renewed; we have—

$$(396 \text{ m.} - 347 \text{ m.}) \div 7{\cdot}78 = 6{\cdot}58 \text{ metres},$$

as mean acceleration and retardation of sound, and of the atmosphere through its whole height by wind. It is—

$$347 \text{ m.} - 51 \text{ m.} : 347 \text{ m.} + 51 \text{ m.} = 1 : 1{\cdot}37$$

as the oscillations of the magnetic needle within the mean of the two regions of intensity, the mean of the minima to that of the maxima of magnetic intensity being $1 : 1{\cdot}37^2$.

The apparent irregularities in this system of compensations corresponding to $1 \div 59{\cdot}56$, the eccentricity of the orbit of the earth, or the mean tension of all ascending vapour against the weight of the atmosphere, or to—

$$1 \div 59{\cdot}56^2 = 1 \div 3546$$

the mass of the sea to that of the earth, &c.

The compressibility of water at 15·22° C. by one atmosphere being 0·0000464, the velocity of sound in water is, according to Laplace's $\sqrt{g \div \lambda}$, equal 1454 metres.

The earth below the atmosphere and sea is a solid, with an intervenient more or less liquid stratum, in the mean 5·45 times more dense than the salt sea. Attraction within changes with distribution of density and depth. The change in solar distance is—

$$1 \div 29{\cdot}78; \text{ it is } \sqrt{29{\cdot}78} = 5{\cdot}45:$$

when we assume as probable that the mean of compressibility of the substances composing the earth is under pressure of one atmosphere $1 \div 29{\cdot}78$ of that of water, considering differences of temperature, o. s. p. as $1 \div 29{\cdot}78$, is the percentage of water-stuff in the air or of salt in the sea, the velocity of sound for the mean earth will be 5·45 times that in water, or 1454 m. × 5·45 = 7933 metres.

The velocity of a point at the equator in daily rotation is 457 metres, and its centrifugality is $1 \div 289$ of gravity. A tangential velocity of 457 m. × $\sqrt{289} = 7933$ metres, or that of sound in the mean solid would make any particle all over the earth absolutely centrifugal if the pressure of the atmosphere and sea, and adhesion and cohesion of molecules in and amongst each other, the centrifugal opposition and the co-attraction of the sun, o. s. p., did not turn projection into

vibration. All sound carried to the centre of the earth is neutralised by a counter-vibration from the opposite side.

The compressibility of ocean water at its mean temperature is 1÷12·08 (the proportion of the polar zones to the sphere) less than that of water at 20° C. (the mean temperature of the surface layer of the sea), or only—

$$0{\cdot}0000417 = 1 \div 23730$$

of the depth of the ocean, which is also the proportion of the radius of the earth to the mean solar distance. The compressibility of the ocean by the atmosphere stands thus in a simple relation to the elliptic and polar motions of the earth, and to the elastic reactions of land, shell, and intervenient stratum, in an opposite sense to those atmospheric changes for which the sea originally furnishes the material.

When sound is collected and levelled by the tympane of the photophone, and entrusted to beams of light and radiant heat, centralised by a lens, the air-waves produced by our breath are transferred from the general atmosphere to the atmospheres and bodies of molecules, and re-transferred to the receiver again magnified from molecular to terrestrian dimensions.

When, in the telephone, electric and magnetic motions are substituted for plain light and heat motions of the photophone, the difference is one of inter- and intra-molecular motions. The again enlarged sound-waves are delivered to and re-centralised and insulated by the ear-tube, which transmits the message of the outer world to the molecular electric conduction of the acoustic nerve for conveyance to the brain,—the camera where we store and arrange, to the degree of its capacity, all phonographs and photographs by which we refract, reflect and concentrate, diffuse and absorb the intelligence of the infinite.

"La différence qui existe entre les sons graves et les sons aigus est si frappante pour nos organes qu'elle doit certainement correspondre a quelque modification physique bien caractéristique dans l'air qui porte les sons," said Pouillet, without fathoming his thought to its full depth. The deepest audible wave has in the mean state of the atmosphere a length of 10·36 metres, which is that of a column of water equal in weight to the whole atmosphere pressing on us. The highest audible sound-wave is 31 m.m., the 1 ÷ 333 of the deepest, 1 ÷ 333 being the mean centrifugality of the earth, and the mass of the atmosphere to that one of the sea, o. s. p.

Sound is not transferable through vacuum. The waves

of matter to be audible must be of a certain size, which is directly obtained only by the mass motion of matter against coherent or contiguous matter. The vibrations of a tuning-fork within a vacuum are inaudible, yet they put the bell around in vibration, for they are seen.

The denser a medium at equal elasticity, or the more elastic at equal density, the more rapidly it carries sound. But when sound is carried by light it travels still more rapidly, and yet it shall then be carried by a luminiferous ether infinitely less dense than our atmosphere.

The emission, as well as the undulation, theory has occasionally predicted observations, for truth may be disguised in myths. The propagation of light does not require what we do not know, but atoms about which we know a little. Whereas Newton left it "to the consideration of the reader if his agent," leading matter according to the law of gravity, "be material or immaterial"; the apostles of ether now call it matter. If it be matter in any state the propagation of light should, like that of sound, be connected with the *g* of the earth. Yet light is said to travel uniformly across the orbit of the earth from the moons of Jupiter and the most distant stars, and from any generator to any receiver whatsoever, unless within our atmosphere. Yet attraction vastly decreases through these spaces, and depends on mass. The velocity of light should also be influenced by the *g* of the mass from which it starts. The density of ether—indifferent if it be gaseous, liquid, or solid—could not be uniform between the generator and receiver of light.

According to some the ether is an undivided rigid solid. How could it undulate, having no internal elasticity, no space for motion? how could it be pierced regardless of the propelling and resisting force, according to the theory of emission?

The velocity of sound is measured by elasticity of tangible matter; but who shall measure the elasticity of what nobody ever felt, saw, and caught, or intelligibly described?

According to others ether is a contiguous liquid, moving with "infinite" rapidity in vortices within bounded spaces, now carrying all sorts of matter along, now passing through it, now surrounding it through vastest spaces with undensest density, now jammed and compressed between its molecules.

The liquid shall, revolving in closed spaces or in a finite universe, become an absolutely rigid liquid. A number of entertaining experiments shall even prove this stupendous theory. A bounded universe within infinity is an absurdity;

and what gives rigidity to a whirling liquid within a bounded space but centrifugality reflected, which is the outcome of centripetality, of plain matter? Centrifugality cannot produce an effect greater than its cause: a liquid at the limit of absolute rigidity by vortex motion would either break its boundaries or destroy its own motion, collapse; and centripetality, and consequent polarity, would be re-established by increase of density of the one part, and creation of a vacuum of the other, inside a bounded space.

Can one not see that a gas, a liquid, a solid, anything having no interstices,—that is, no inner surfaces, no disparities of inner motion, but filling all space with uniformity,—is nothing?

I ask the thoughtful amongst the professors of the infallible creed, with all its sectarianisms, Is there anything more difficult of conception and explanation in action at a distance than in the existence of matter? Any least volume taken as a sphere, not moving, as a nucleus, a point, surrounded by one or more concentric strata of least thickness, is absolutely contiguous matter when the difference of attraction from an interior to the next exterior stratum or surface is less than the difference in excess of size of the exterior over the interior surface, which may represent as well a decrease as an increase of attraction toward the centre of the whole space or volume; but when equality is reached attraction towards this common centre simply increases with the size of the surrounding concentric surfaces, which then represent vacuum. There is no difference between matter and vacuum, but an addition of force with additional space, which may be as well a relative gain as loss of force in proportion to increasing surface of attracting volume, and an absolute subtraction of attracting force equal to the increase of the attracting surface.

If matter was absolutely contiguous, without separating absolute vacuum, if there were no action at a distance there could be no matter; it had to crush itself out of existence to result in absolute rest which—for us finite beings and brains inconceivable like infinity—yet is; vacuum furnishes the possibility of extending motion, which, together with attracting motion, is the existence of something.

We know of light only because we have an organ constructed to make us aware of it. Light appears only where there is matter thus moved that it enlightens our eye. Issuing from matter, radiating all round, light spreads in equal matter in equal manner, and in different matter in different manner with decreasing intensity, or is partly or wholly reflected by opposing matter.

When we hold an opaque object before a flame it disappears, but we see the air, matter, illuminated to the sides of the shadow. When our side of the earth is away from the sun, and the refraction of light by our limited atmosphere no longer carries it to us, when the attraction of the earth no longer finds matter productive of light, the light of the sun disappears until it meets matter, which reflects on us the motion directly received from the sun.

If all space were filled with matter, indifferently how we call it, we ought to see all space illuminated outside the shadow of our earth. When the tail of a comet is matter so attenuated that we see beyond it stars with undiminished splendour, it is visible because the motion communicated to single particles by the sun is transferred by their motion to the earth.

With the ether hypothesis, bridging a chasm by a word, the imaginary only in the undulatory theory falls.

There was a time when heat was a fluid accumulating round molecules; now few physicists will deny that it is a motion of the molecules. Why, then, appeal to phantoms when it comes to light? The apparently uniform velocity of light in space, which great, is not instantaneous, as action at a distance might be supposed to be, and prime gravity is, disturbs the mind. But is not radiant heat the rapid companion of light? Is not sound made the companion of both? Do the ethereans furnish any reasons for this great and uniform velocity of light travelling through the heavens? And if they cannot, what is the value of the idol which does not lead them out of their perplexities?

Light and radiant heat shall be propagated in straight lines, whereas other forms of energy follow the sinuosities of conducting substances. But even this distinction is illusory; the beam of light is not merely refracted and reflected, but also diffracted, bent, and divided.

The smallest, like the largest, portion of matter is an individuality; it has in itself a distribution of force, according to its past and present relations to other individualities which lead to various inward and outward motions.

Examining the nature of these motions, the condition for the existence of matter is attraction; without it, no matter. The consequence of attraction is repulsion, which may be rectilinear and re-destructive, or curvilinear, rotary, and orbital. Matter has temperature depending on its relations to other matter; vacuum has no temperature, but volume, depending—in its definite or indefinite limitations—on the mass, volume, and motions of matter.

There is no light without heat, but there is heat without light. I have defined heat as the rotary motion or tendency of masses and smallest particles, themselves having their density unequally distributed in them, and light as their orbital motion, dividing in waves. There is no light, only heat,—dark light.—when the orbital progressive motion falls entirely within the individual, when the transport of its centre of force in space is less than the mean velocity by rotary motion of any point of the outer surface of the particle, or aggregation of particles.

Emission and undulation theories are not irreconcilable. To see this we need not discuss the "material or immaterial agent," or the ponderable or imponderable ether, or my rejection of either. "One distinguishes two general kinds of motion essentially different, the translatory and the vibratory." "The former is the basis of the system of emission, the latter of that of undulation of light." This distinction merely indicates differences produced, in equal results from equal causes, by the variety of extraneous circumstances in which they are placed and under which they occur. Both motions are due to an attraction and repulsion of one mass by another, both these effects being convertible into each other. The difference consists in the rapid succession of reversions, and the contact or approximate contact of the individual masses and particles.

Matter from space, meteors of all sizes and kinds join the earth, and a regular interchange of mass exists within the solar system, and more or less between systems. We may therefore advance in favour of the emission theory the hypothesis that Crookes's corpuscles are propelled from the sun and stars with the velocity of light towards the earth, attracted, directed, and concentrated by her through a polar interaction of projector and receiver, without there being any need for a fourth state of matter of which we have no concept.

I call the action polar because it cannot exist outside the profiles of projector and receiver, without whose attraction the centrifugal energy of the projector, or particles of his, would not acquire the trajectory surplus necessary to start particles away from its own re-attraction and polar action, the mutual attraction and energy between both masses being zero outside the cones connecting the semi-surfaces of generator and receiver, in so far as there is no third centre of force affecting both.

Striking the atmosphere, meeting with resistance, the gradually penetrating projectiles produce light and radiant

heat, which are propagated by the mass struck as waves, as vibrations. When light stops reflected or absorbed, as all become radiant heat, this heat turns, gradually advancing, into conducted heat.

Further, supposing that planets and moons project out of their comparative darkness by corresponding causes, under the far more powerful attraction of the sun, a return current of particles with velocities far exceeding that of the particles moving against them, they maintain the lighting and heating process and power of that centre.

I shall not detain the reader with the proof that the hypothesis is not admissible.

Light and heat arise from changes of frictional and chemical character in masses and molecules,* the resulting motions inducing, by way of gravity, corresponding motions not only in adjoining bodies, but also through vacuum, at a distance, in molecules and masses.

The increase of heat and the following generation of light will always consist in an increase of molecular centrifugality somewhere, compensated by contraries elsewhere. All changes of temperature and production of light are therefore inter- and en-molecular motion.

The sun overrules all light and heat processes within its system. It yields light to its own and other systems; it induces in its planetary following a harmonic motion and life. It generates heat and light by alternate composition and decomposition of atoms and molecules embraced in a progressive combination, metamorphosis, and condensation from constitution to re-dissolution, from birth to death; this process in its generality being induced by planetary motion round the sun, and with the sun in its orbit, not without reciprocity with other systems.

When stellar, solar, or terrestrian molecules experience by any cause an elevation of centrifugality, wholly or in portions of them, we feel the propagation of such motion as heat, and when the increase is of a certain amount, and results in a trajectory, we see it as light. Not only the light spending portion of such molecule gets more or less removed from its centre of force, but the whole molecule is moved from the centre of the portion of mass, or of the mass to which it belongs; it tends instantaneously to absolute centrifugality, to be in the next instant repelled by a distant mass and by its neighbour molecules, whose centrifugality

* My views on adhesion, cohesion, and chemistry are chiefly contained in "Heat and the Atom," "The Origin of Organisms" (1859), &c.

and consequent counteraction it increases with regard to the centre of its own mass; it propagates light wherever it meets with resistance, with matter which has to accommodate itself to the polar and multipolar motions of molecules, to their inter- and en-molecular axes, faces, and stratifications, and finally turns to heat entirely absorbed.

Be S the sun; *a*, an atom of his involved in a light generating combination, repelling it from S; *c*, a molecule of a satellite, L, of Jupiter turned towards *a*, and not separated from it by opaque masses; and *b*, an atom of the earth turned towards *c*: *a*, in itself a microcosmos, is repelled from S and brought nearer *c*; it is therefore more attracted by L than the mean S, and therefore next exercises an increased repelling and frictional influence against *c*, which thus repels and presses interior molecules, *f*, towards the centre of L; *c* describes an orbit towards the centre of L. This motion puts to greater rotation the more resisting, denser *f*, which, thus induced to greater rotary motion, renders only part of the acquired orbital-motion as reflected motion, as light towards *b*. But all this transmission of orbital motion of waves, this propagation of light and radiant heat, requires time,—a completed circuit of coming and going motion between whole strata of generator and receiver.

Sound does not reach, like light and heat, through space; it is not heard through vacuum, but it can be more or less communicated by any matter contiguous or made contiguous. The spaces dividing heavenly bodies are therefore empty spaces; even light and radiant heat carry no message from them to our ear, but they carry it through our atmosphere.

Sound is the result of a propulsion and repulsion originated by that attraction which puts the striking body in motion. The amount and quality of shock, and the qualities of generator and receiver, determine the qualities of sound; thus the distance, power, and qualities of generator and receiver of light and heat determine their quantities and qualities.

The velocity of sound does not depend on the shock which produces it, but on the medium in which it is propagated; any over motion, for instance by wind, being a transport of the medium itself with its vibrations, or a mutation from sound to silent different motion, again mutated to sound. The velocities of light and radiant heat also depend on qualities of the medium in which they travel, and most media even exclude the propagation unless reduced to less

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than a contiguous film, or to the gaseous state. Why, then, is it that light and radiant heat seem to use time in proportion to the vacuum distance separating heavenly bodies, the resistance being zero? Because vacuum is in its dimensions determined by the interaction of aggregated masses of matter, of generator and receiver. Vacuum is a medium having the quality, distance, rectilinear dimension (no other), transverse undulation, cross section of orbits, making the orbits' waves only existing where there is matter.

We know of sound only because we have an organ which has adapted itself to its reception and valuation, and I showed the motions producing sound in definite relations with the inorganic earth and with the grouping of its chief sections. As we also have an organ so developed and constructed as to feel and value light, and our whole body is made dependent on heat, we may well assume that the motions constituting light and heat stand in similar relations to the mass, structure, and motions of the earth.

Any particle is subject to the common attraction and repulsion by the earth; it is next subject in common with the earth, and again as individual, to the differentiating attraction and repulsion by the sun; and it is further subject to the attraction and repulsion by other bodies, molecules, and atoms, non-terrestrian and terrestrian, themselves having their special centres and internal distribution of force.

The earth is a solar particle separated by vacuum from the sun and other heavenly bodies; it is out of equilibrium with regard to the central mass. This constitutes differences in the light received by the earth from without, and generated within its limits, where the origin is entirely surrounded by neighbours coming in contact with it.

The sun is the immediate ruler of the earth. Any terrestrian molecule entering on the motion light, indifferently if received from outside or inside the earth, is for an instant subjected to a tendency of absolute centrifugality from the earth, immediately reacted against; it is therefore an object in contention between the centrifugalities and attractions of sun and earth, all third disturbing parties and light generators being subordinate to their leading contest among themselves.

There are solids more or less penetrated by light and radiant heat, themselves similar, yet distinct in action, the difference being orbital and rotary motion, one passing into the other as arrested transport into accelerated rotation, and arrested rotation into accelerated transport. Of these solids none but solid water is of any frequency, and all but ice are

so hid away in the general solid that we may well say light and even radiant heat are absolutely reflected by the solid earth; they do not penetrate except as conducted heat, with vibrations whose axes are less than the mean radii of the molecules.

Even liquids, the leading one again being water, are as often an obstacle as a medium for their propagation; undulation and actual transport get confounded. The true medium for light and radiant heat, making them two in one, is vacuum, in which they are zero, distance, and matter in the form of gas, and foremost that great mass of gases we call the air, the atmosphere itself being—by its ascending vapour of water—a still more rapid, or the real, propagator, and by its water, in the form of vesicles, drops, solids, and vapour in absorption, an obstacle to the rapidity and effectiveness of propagation of light and radiant heat.

That relative quantities now to be used, and described in this causation in "On some Properties of the Earth," have been obtained without any regard to sound and light, but with reference to gravity and heat, is in favour of their correctness and of my views.

We have to consider the limited earth as an individual of a certain mass, density, shape, and inner distribution of force, *i.e.*, matter; and any planet, moon, comet, and meteor, and any star and nebula exchanging light and heat with the solar system, as another individual. Such individual is, in the vacuum of the heavens, a medium for the propagation of outside and inside light and heat, consisting itself of various media. And as the velocity of light is not the same in different media, it will, without time in vacuo, not act beyond vacuum in equal time on those individuals; and even with regard to the same individual, indifferently from where it is generated, or reflected and re-transmitted, the time which elapses before it becomes sensible for the receiver will depend on the rectilinear interaction of the generator and receiver, or on their distance and change in distance.

The ear is a terrestrian organ; it finished with the velocity of absolute centrifugality of a particle of the earth: the eye is a solar terrestrian organ, and this velocity enters as a first term in the velocity of light; it is the velocity of resistance to solar differentiation and induction, productive of light within the medium earth; and as with the earth so with other bodies, and with any particle according to its microcosmos.

Light cannot be generated where there is no atmosphere and no liquidity from which it is developed, no actual

molecular transport; it can at best be reflected, and re-transmitted, and even then the solid molecules reacting upon outside matter as re-transmitters of light must assume a centrifugal tendency, developing a molecular atmosphere and sea, &c.

When any particle is on the side towards the sun, and the latter, then acting negatively on it, gives it alone, or in common with other heavenly bodies or purely terrestrian molecular combinations, all under solar supremacy, the tendency is towards the velocity $\sqrt{g r}$ of absolute centrifugality; this tendency increasing molecular rotation, heat, opposed to plain gravity, g, will move it from west to east. Immediately after, however, the particle lifted towards the sun, made more solar, receives a tendency to an accelerated motion in an orbit round the sun from east to west, to a velocity greater than the mean one, B, of the whole earth in her orbit round the sun, and therefore in advance of the earth, and opposed in direction to the tendency $\sqrt{g r}$ of the particle. Terrestrian and solar centrifugality at once reacting, the former against solar attraction and lifting, the latter against terrestrian centrifugality moving the particle away from the sun in front of the earth, or so tending, and actual transport of the particle being thus prevented, there will be vibration only according to the qualities of the stratum of which the particle forms part, and the qualities of the particles and of the enclosing and underlying strata, the resistance of less elastic against more elastic molecules. All the qualities of strata will possess certain respective proportionalities as long as the contiguity of the terrestrian mass is not entirely destroyed,—as we may observe it with comets when they are dispersed into nebula and split into separate masses.

When the particle is on the side of the earth away from the sun, and therefore receives the tendency towards the velocity $\sqrt{g r}$ only by reflected light (action), or by own light from third bodies outside or inside the limits of the atmosphere, the tendency will still be in the sense of rotation of the earth, but east to west, and the particle lifted from the earth will have the tendency to slacken its motion round the sun, and to stay behind the mean motion, B, of the earth round the sun. Earth and sun, however, now combining in re-attracting the particle towards the earth, the mass of both being more or less between the particle and the sun, there will be again an opposite tendency of motion of the confined particle, a wave motion, propagating the impression from the respective light generator. We

may therefore say, *in a general way*, the censed velocity of light *in vacuo* will, with regard to any insulated mass within the solar system, depend on the radius of the receiver into solar distance, indifferent if the inducer of light is in or out of the system; this quotient being subject to change when the body is unconstituted, like a comet which expands indefinitely and breaks up in individual masses.

When we, then, naturally ask, What is solar distance, which varies? what is the radius of the earth? does it include the height of the atmosphere and the mean depth of the moving sea, or the mean depth of sea and land raised, or neither? we perceive at once that the first relation given is, indeed, a general way, and that the censed velocity of light depends essentially on the receiver, without being independent from the generator, and its system, ruling all other generators and reflectors with regard to our observatory, the earth. We may comprehend that fully by putting to us the further question, How could we receive an image, how could we chemically impress it in a photograph, if the general, the sectional, and the (in themselves) microcosmical molecular masses did not exact on us distinct impressions of their individual existence and motions?

We would be, indeed, at a loss to make anything of the velocity of light if our earth were not so permanently constituted, for the time being, with regard to the outside world, and especially the sun, as our sound eye is,—a colour-blind unsound eye combining either exceptional wave-lengths or exceptional velocites, and therewith incorrect numbers of vibrations. The eye is, in the mean of the indices of refraction of its parts, identical with that of water; and salt water, the atmosphere of the earth, is that of the eye, which so corresponds in its characteristics to sea and land.

The atmosphere, the real home of light and radiant heat, is the distinct stratum at the limit of which begins the action of light and radiant heat.

I showed the air—the elastic, gaseous, dielectric cushion round the earth—to be the centrifugal outflow of the sea, its $1 \div 333$ in mass, as much as the mean centrifugality is of the mean attraction of the earth. I showed that the vapour ascending from sea and land against the pressing air, turning it into atmosphere, is $1 \div 59{\cdot}56$ of the mass of the atmosphere, and that all the water-stuff ascending, and again descending as vesicles, drops, crystals, and absorbed vapour, at any time suspended in the air being—

$$1 \div 59{\cdot}56 \times 2 = 1 \div 29{\cdot}78 \text{ of the atmosphere,}$$

represents the solar disturbance in the stratum air, this water-stuff, all originating from the sea, being lifted and carried in orbits through the air by the differentiating power of the sun.

I showed that the atmosphere is on the day-side by the reaction of solar centrifugality and terrestrian attraction against solar attraction and terrestrian centrifugality, and on the night-side by the joint attraction of sun and earth against the centrifugal tendencies of the earth confined within a definite boundary by an outer atmosphere.

The ascending vapour, the $1 \div (333 \times 59{\cdot}56)$ of the mass of the sea, is thus confined between a gaseous skin and a liquid and solid stratum. Circulating in the air the water-stuff, with its varying forms, becomes the conveyer and distributor, the propagator and the obstacle, of light and radiant heat in the atmosphere,—that is, of the action of the mass, atmosphere, and molecules of the sun, &c., on the mass and molecules of the outside earth.

We thus have the initial or vacuum velocity of light at the outside limit of the atmosphere, B representing the mean velocity of the earth in her orbit round the sun.

Formula *a*:—

$$\sqrt{(\sqrt{g r \times B}) \div (1 \div 333 \times 1 \div 59{\cdot}56)} = \sqrt{(\sqrt{g r \times B}) \times (333 \times 59{\cdot}56)}$$

$=$ A, the velocity of light in a second $=$ 305,686,092 metre.

Do the following coincidences not offer a strong probability in favour of my reasoning? I showed (o. s. p.) that the fresh sea is $1 \div 3546$, and the salt sea $1 \div 3426$, of the mass of the earth, and that the earth has in either sense, equatorial and polar, apart of the flattening, an excentricity of $1 \div 3546$. Observation gives the index of refraction of air at 0° C., under 760 m.m. pressure, 1·000294; when we take the air with the water-stuff and the 15·22° C. it is—

$$1 \div 3426 = 0{\cdot}000294,$$

the $1 \div 3546$ giving $= 0{\cdot}000282$.

(To be continued.)

II. THE POISONING CASES OF THE LAST HALF-CENTURY.*

THE stories of Tawell, of Palmer, of Dove, of Madeline Smith, which in their turn filled the columns of the press and set in action the tongues of half the kingdom, have not yet lost all their meaning. If we carefully read the book before us, we may see that it contains lessons of importance not merely to lawyers and physicians, but to much wider circles. The moralist may, for instance, be painfully struck with the startling cross-lights which a great crime throws upon society, and with the strange revelations which they bring. How many persons in the little town of Rugeley, besides Palmer, moved in the light of respectability, perhaps even in the odour of sanctity, until the trial of 1856 enabled us to look behind the scenes!

A much more important consideration, from our point of view, is that the heroes of the poisoning cases before us belonged, almost without exception, to the so-called respectable classes. Their previous conduct had not, indeed, been morally free from reproach, and several of them had been concerned in doubtful transactions. But they were not habitual criminals; they were not the companions of criminals, and they could not rank in that order of men who are "well-known to the police," and who are apt to be suspected in case of any untraced crime. One of them, indeed, could and did say "My station in society places me beyond suspicion!"

This point naturally brings us to the question of their education. Of the thirteen persons who have thus been placed before the tribunals on the charge of murder in so cold-blooded and treacherous a mode, four were members of a learned profession; none, probably, were grossly ignorant, and none had in their early life been brought up in oblivion of the distinction between right and wrong, or systematically trained to a general warfare against society.

These facts, surely, are not without bearing upon the

* Reports of Trials for Murder by Poisoning with Prussic Acid, Strychnia, Antimony, Arsenic, and Aconita, including the Trials of Tawell, Palmer, Dove, Madeline Smith, Dr. Pritchard, Smethurst, and Dr. Lamson, with a Chemical Introduction and Notes on the Poisons used. By G. LATHOM BROWNE, Barrister-at-Law, and C. G. STEWART, Senior Assistant in the Laboratory of St. Thomas's Hospital. London: Stevens and Sons.

moral and social influence of education, and the benefits to be hoped from its universal extension.

The question of heredity here naturally occurs. Had crime or notorious vice existed in the families of these evil-doers; and were they what they became by the right of descent? On this most important point the trials throw no light. Thus concerning the parentage and ancestry of Tawell we learn nothing. About Palmer authorities are almost equally silent, and the same may be said as regards the other persons convicted. To obtain the needful information might now be difficult, and it is quite possible that its publication, if obtained, might prove a very costly luxury. We can only, speaking from a physiological and psychological point of view, suggest that in case of all notorious criminals the career of their ascending and collateral relatives should be traced out and put on record as a sequel to the trial. Concerning the genesis and the propagation of crime more abundant and more accurate data are wanting, before it will be possible to theorise with acceptance and success.

Leaving, however, all the above-mentioned matters to be discussed elsewhere and by others, we come to the main consideration, How does our modern chemical and medical science stand affected by the trials here recorded? Have any doubts touching its competence arisen, either in the mind of the public, of the legal profession, or in that of men of Science themselves? In other terms it may be asked—

1. Have we at command means for the detection of all known poisons under all circumstances?
2. Have our leading toxicologists and physicians shown sufficient skill in the application of such means?
3. Is our manner of conducting criminal trials, and of obtaining and dealing with the evidence of experts, well adapted for the discovery of truth and for the elimination of error, or could it admit of useful modification?

Before attempting to discuss any of these questions we must make the preliminary admission that not all poisons are known. Many natural productions, and still more artificial compounds, have not in this respect been even approximately studied, and in England, at least, such researches are now rendered almost impossible. We dare not even affirm that all existing types of poisons are known. Indeed from time to time reports reach us of substances which in their action simulate malignant fever. Leaving all such considerations aside, as not immediately apposite,

let us take up the first question, and reply with an emphatic "No!"

Aconitine is an instance in point. We may judge that it has been taken by the symptoms which it produces, and by physiological tests which the operator applies to himself or to other animals. But true chemical reactions, precise and available, it does not exhibit. We may extract it from the remains of the deceased, and obtain it, freed from all other substances, in a state of purity; but when this is done we cannot give a chemical or physical fact in answer to the question, "How do you know that this particle of matter which you produce in the watch-glass *is* aconitine?" The microscope, the spectroscope, the polariscope, which have solved so many problems, are here silent. Colour-tests have been proposed, but a doubt hangs over them all, and in some cases the reaction described would seem due not to the aconitine itself, but to some impurity, which may or may not be present. Hence they are of no scientific value.

We may next turn to the ptomaines. Recent researches have made known the existence of a class of alkaloids or organic bases which are generated in the body during putrefaction, during morbid conditions prior to death, and even, it is said, during normal healthy life. Concerning these cadaveric alkaloids we know, as yet, sadly little. We cannot state their number, nor lay down their physiological action, the circumstances under which they originate, and the reactions by which they may be recognised. Some of them resemble strychnine in their action upon the human system, and in certain of their chemical reactions. Indeed, in the work before us, mention is made of a case where an Italian toxicologist, F. Ciotto, pronounced strychnine to be present in a dead body, whilst Prof. Salmi, one of the chief investigators of the ptomaines, being called for the defence, showed that the substance in question differed in some of its properties from strychnine, and was probably one of the ptomaines. Some time ago it was asserted that the ptomaines might, as a class, be readily distinguished from the natural vegetable bases by their property of reducing potassium ferri-cyanide to ferro-cyanide, and thus giving a blue precipitate with ferric salts. But some of the vegetable bases—among others *aconitine*—produce the very same reaction, though more slowly. Hence the class distinction is broken down, and all that can be done is, as the authors of the work before us suggest, to depend upon the fact that there is no ptomaine known which produces *all* the characteristic reactions of morphine, aconitine, &c. But this is

by no means a satisfactory state of things, and an immensity of delicate and difficult work will be needed before we can reach actual certainty.

It is manifest, as was actually shown in Lamson's case, that the existence of these ptomaines suggests a new and plausible line of defence, of which counsel will not be slow to avail themselves. But there is a further point as yet overlooked. A physician, or a chemist, or a man tolerably skilled in experimentation, may prepare some of the ptomaines from animal matter, and may administer them to any person he wishes to destroy. How is his crime to be detected? Suppose the ptomaine is found in the corpse of the victim; the defence at once admits its presence, and argues that it must have been formed there either prior to or after death. The prosecution will certainly fail to prove to the contrary.

But coming down to much less difficult subjects, we find in the statements of the experts engaged in these trials, and in the authorities quoted in this work, a manifest lack of that agreement which must follow from accurate knowledge. Let us take the question, How long after death can hydrocyanic acid, administered just previously, be recognised? Mr. Allen affirms that its detection is rarely possible after twenty-four hours. On the other hand, "Casper obtained from a corpse more than 18 milligrammes eight days after death; Sokoloff detected it in hounds sixty days after; Dragendorff after four weeks in a dog, after eight or ten days in a man. Reichardt found it in a body two months after death—in the solids, though not in the urine."

In the Tawell trial the odour of hydrocyanic (prussic) acid seems to have occasioned much wrangling, and many conflicting opinions were given. Counsel asks Mr. Cooper, "Do you agree with Dr. Taylor that the odour of prussic acid may be found when all tests fail?" Mr. Cooper replied, "I have no doubt that prussic acid may exist without being smelt." Mr. Norblad, a surgeon and apothecary, of Slough, further deposed that he had tried experiments with this poison upon dogs, and on opening their bodies eighteen hours after death could not discover any odour. He had further mixed 12 grains of prussic acid with a pint of porter, and could not smell it. On the other hand, one Henry Thomas, a druggist's shopman, whose claims to rank as a man of Science may possibly be called in question, did not think porter would disguise the odour. He had mixed 30 drops of this acid with 11 ounces of porter, and the smell was "slightly perceptible." On another occasion he

had put 20 drops of the acid down the throat of a parrot, with a glass syringe, and the smell was so suffocating that three women who were present had to leave the room! On opening the body of the woman poisoned by Tawell, Mr. Champneys positively smelt prussic acid, whilst the other two surgeons present could not. In a case of suicide by potassium cyanide (*see* "Chemical News," 1861, p. 261) the smell of prussic acid was not perceived by the surgeon during the *post mortem*, nor by the chemist during the analysis, until the contents of the stomach had been distilled with dilute sulphuric acid. In experiments made by the author a poisonous dose could be distinctly smelt in 1½ pints of stout.

Now making all due allowance for the great difference in the sense of smell in different persons, for its want of constancy in the same person, and for the admitted variation in the strength of commercial samples of prussic acid, it may still be urged that there is here an undesirable want of agreement on a simple matter of fact.

We pass next to strychnia. In the Palmer case Dr. Taylor failed, as is well known, to detect this poison in the body of Cook. Commenting on this result Mr. Herapath said, in his examination, "If it had existed in the body of Cook it ought to have been discovered,"—"if it was there Dr. Taylor ought to have found it," and he would not deny having even said "No doubt strychnia had been given." On the other hand, Dr. Taylor denied that strychnia could always be found when it had occasioned death.

A further difference of opinion took place as regards the persistence or the transformation of strychnine in the body. The late Dr. F. C. Calvert succeeded in extracting strychnine from the bodies of some hounds three weeks after their death. Mr. Nunneley, without pronouncing strychnine absolutely indestructible, maintained that within ordinary limits it could be found unchanged, even forty days after death. Richter has even detected it in animal tissues *after eleven years*. On the other hand, Mr. Morley leans to the opinion that the poison, having done its work, ceases to exist as such. Dr. Taylor, too, holds that it "undergoes some partial change in the blood." Dr. Taylor, at this memorable trial, even committed himself to the statement that "the colour tests [for strychnine] are fallacious," whilst they are now recognised as decisive if the known precautions are observed. The same eminent toxicologist declared that he knew of "no process by which strychnine could be

detected in the tissues." This has been repeatedly done, as Dr. Taylor has since admitted.

In summing up all that has preceded we must remember that the art of toxicology has made rapid strides since the trials of Tawell and Palmer, but that scope for improvement still exists.

Turning for a moment to the strictly medical evidence given, in which the cause of death is inferred from the symptoms before death and the appearances afterwards presented, we find an amount of discrepancy, and even antagonism, which is positively painful. If we confine ourselves to Palmer's trial we find the death of Cook successively ascribed to poisoning by antimony, to tetanus (traumatic or idiopathic), to apoplexy, epilepsy, to convulsions with tetanic complications, to angina pectoris, and to the effects of morphia. These varied hypotheses, brought forward by persons who could judge only by hearsay, elicited from counsel on both sides in turn most severe comments. Said the Attorney-General (Sir A. Cockburn), in his reply to the defence in Palmer's case, "To me it seems a scandal upon a learned, distinguished, and liberal profession that men should put forward such speculations as these, perverting facts and drawing from them sophistical and unwarrantable conclusions, with the view of deceiving a jury. I have the greatest respect for Science,—no man can have more,—but I cannot repress my indignation and abhorrence when I see it thus perverted and prostituted to the purposes of a particular cause in a court of justice." Again, with reference to the suggestion that Cook had died of *angina pectoris*, he speaks of the "ignorance or presumption" of the witness in question. "I say ignorance or presumption, or, what is worse, an intention to deceive." One more quotation will suffice:—"I abhor the traffic in testimony to which, I regret to say, men of Science sometimes permit themselves to condescend."

It is, indeed, deeply to be deplored that the slightest room should have been given for comments such as these. The effects of such denunciations, and of the exhibitions by which they were called forth, have even yet not entirely disappeared. But did counsel and judges ever ask themselves what lies at the root of the party-feeling and the unscrupulousness thus complained of? Of this hereafter.

We have yet to inquire whether the analytical chemists engaged in these poisoning cases employed all the known precautions for eliminating error, and whether, again, they observed that discretion which a respect for public justice, for

their own professional honour, and for Science should dictate? To neither of these questions can we reply in the affirmative.

We refer here to the admission of no less distinguished a chemist than Prof. Brande, that in applying Reinsch's test for arsenic he generally used a halfpenny rolled down as "pure enough for the purpose." Dr. Taylor, also, seems for twenty years to have used untested and possibly arseniferous copper for the same test. Dr. Herapath, even, in a letter to the "Times" in reference to Dr. Smethurst's case, though not written until after his conviction, pronounced the whole set of operations in that case a "bungle." "Taylor must have known that Reinsch's test was inapplicable in presence of nitrates or chlorates." Leaving these instances of something closely bordering upon carelessness, we cannot close our eyes to the personalities contained in Herapath's comments on Taylor,—strictures so unworthy of a gentleman and a man of Science that we could almost suspect some old grievance to have existed between the two. The verbal communications made by Dr. Taylor to one Mr. Augustus Mayhew, of the "Illustrated Times," who succeeded in "interviewing" him, professing to be employed by some insurance company, were, to say the least, highly indiscreet, and became unpleasantly notorious. Nor did Dr. Taylor's subsequent letter to the "Lancet" greatly improve his position. It contained a passage from which counsel sought to extract the meaning that the conviction of Palmer was necessary for the public welfare.

We now come to our last question, and ask whether the English method of dealing with the testimony of experts is well adapted for eliciting the truth and eliminating errors due to possible ignorance, carelessness, or bias on the part of the witness? Legal authorities, in this country, we believe, uphold the present system, or did at least until quite lately. Thus Mr. Justice Stephen, in his "History of Criminal Law in England," writes, with reference to Palmer's case, "A study of the case will show . . . that the subjection of all the witnesses, and especially the scientific witnesses, to the most rigorous cross-examination is absolutely essential to the trustworthiness of their evidence."

We shall perhaps understand Mr. Justice Stephen's point of view the better if we quote some observations made by him at the Leeds Assizes, late in 1880 or early in 1881. The learned judge is reported to have said that "Leeds was the only town where he never heard those unseemly disputes between the legal and medical professions

which occurred constantly at other places. The leaders of the medical profession in the School of Medicine at Leeds had set an admirable example for many years past of truth and candour, and straightforwardness in the witness-box, and he was happy to see that their example was being followed by the younger members of the profession. When a man really tried to tell the truth, the whole truth, and nothing but the truth, in plain and simple language, notwithstanding what consequences might be drawn from it, or whether he was called on one side or the other, bullyings in Court and things of that kind ceased at once." We cannot help feeling, with a correspondent of the "Journal of Science," that in these remarks Mr. Justice Stephen takes a one-sided view of the case, and that, though possibly without such intention, has gravely wronged the medical profession, and other men of Science whose misfortune it may be to have to give evidence in a court of justice. That experts have occasionally been deficient in truth and candour we admit; but we must at the same time maintain that counsel often seek to prevent the truth, the whole truth, and nothing but the truth from being told, if they believe that it may tell against their clients. Instances of this kind may be found in the book before us.

The expert, "called" by one side or the other, is strongly tempted to become a partizan. He holds an anomalous and unrecognised position between that of a witness and an advocate, and unless he be a man of sterling integrity he is tempted to follow the example of counsel by the *suppressio veri* and the *suggestio falsi*. He cannot help knowing that if he contributes in any marked manner to the success of "his side," his reputation and emoluments are increased. Even if he eschews all such considerations, and adheres absolutely to the truth without fear or favour, he is treated by the "opposite side" as a hostile partizan. So unpleasant is this position that it is recognised among men of Science as a difficult task to come out of the witness-box without injury to their credit and without having received insults for which no redress is possible. Hence, among scientific men, the general opinion is that the expert should be the adviser of the Court, not called by either side, and consequently free from all suspicion of partizanship, since acquittal or conviction would be to him alike.

If we glance over Tawell's case we cannot fail to see how much it would have been simplified had Mr. Cooper, as Assessor of the Court, been empowered to dismiss at once as an absurdity Sir Fitzroy Kelly's theory of the prussic

acid found in the stomach of the deceased having been derived from apple-pips. In many cases, too, medical or chemical facts would be understood more clearly by the jury if presented to them without the intervention of persons who—as may be seen on careful consideration of the questions in cross-examination—either in reality misunderstand the witness or seek to force a meaning upon his words which he had no intention of conveying.

This consideration ought to impress upon experts in drawing out a report, and authors in writing any book which may be quoted in a court of justice, the necessity of extreme caution. Not only is it necessary to avoid every exaggeration, distortion, or misinterpretation of facts, but every word should be weighed, so that it may neither be honestly misunderstood nor be dishonestly misinterpreted.

III. BENJAMIN FRANKLIN AND THE ORIGIN OF LIGHTNING RODS.

By Colonel A. PARNELL, R.A.

THERE is perhaps no important social subject on which educated persons in general, and scientific educators in particular, have such confused and indefinite conceptions as on the operation of that great natural agency which we in England, and our cousins in America, give the vague meteorological appellation of *lightning*, and our French neighbours designate under the more distinctive and accurate term of *thunderbolts*. The investigation of the action of thunderbolts forms a branch of the study of Terrestrial Electricity, a science that necessarily examines and endeavours to assimilate the nature and functions of all those natural phenomena in which, as the result of patient observation, the force of electricity may reasonably be held to take part. The wonderful, and indeed awe-inspiring, manifestations of thunderbolts, earthquakes, whirlwinds and waterspouts, auroræ, meteors, fireballs, St. Elms's fires, "earth currents," and the earth's magnetism, are thus all elements of this most fascinating, and yet most neglected,

portion of Physical Science. The present writer, who, during the last few years, has been able to devote a considerable portion of his leisure time specially to the subject of thunderbolts, and to amass much information in regard to facts and opinions connected therewith, being now engaged in compiling a work on Terrestrial Electricity, has thought that perhaps a few remarks bearing on the relation of Benjamin Franklin to the origin of so-called *lightning conductors*, and arising out of the studies he is pursuing, may be of service in paving the way for a clearer and more definite idea, on the part of interested readers, of the history of these instruments, and of Franklin's original intentions when he introduced them.

Benjamin Franklin was born at Boston, in British North America, in 1706. By trade a printer, his acute and enterprising mind did not permit of this particular calling engrossing his whole attention; for he added to his worldly resources by keeping a shop at Philadelphia for the sale of goods of a variegated nature,—an establishment locally known as a "general store." He appears to have first heard of the new science of electricity at a lecture delivered at Philadelphia by Dr. Spence, in 1746. Franklin was then 40 years of age; but he at once began to investigate the subject for himself, and to make experiments, and it was not long before the same idea that had occurred to Ward in 1708, to Gray in 1729, and to Nollet in 1745, presented itself likewise to his imagination, *viz.*, that lightning was the appearance of an electric spark.

Here, perhaps, we may be allowed to make a slight digression in order to call attention to the confusion constantly made between lightning and electricity,—*i.e.*, between effect and cause. No less an authority than the President of the Society of Telegraph Engineers and Electricians tells us, in his Inaugural Address, delivered in January, 1883, that Franklin, by a bold experiment, "proved that lightning and electricity were one, and consequently obedient to the same laws." In a Lecture delivered at the Royal United Service Institution, on May 6th, 1881, on the same subject, the lecturer said that one of the forms of electricity was "the state of activity (the work-producing state) as in lightning." These *dicta* are in reality tantamount to affirming that a certain portion of the manifestation of the explosion of charge and the charge itself are one and the same thing. A similar derangement of cause and effect is evident also in a paper "On Lightning and Lightning Conductors," read by Mr. W. H. Preece, the well-known electrical engineer, at

a meeting of the Society above mentioned, on the 27th November, 1872, in which paper we are told that electricity is one form of energy, and *therefore* necessarily force. Because, in fact, electricity is a certain effect, therefore it must also be the cause of that effect.

Whilst Franklin was pursuing his electrical investigations he kept up a communication with some scientific friends in England who were interested in his researches. His writings at this time consist principally of a series of letters addressed to Mr. Peter Collinson, F.R.S., and they were eventually published under the title of "New Experiments and Observations on Electricity, made at Philadelphia, in America, by Benjamin Franklin, Esq.," the third edition of which was published in London, by Cave, in 1760. Of these famous letters Dr. Joseph Priestley, F.R.S., in his "History of Electricity" (4th edition, London, 1775), says:—"Nothing was ever written upon the subject of electricity which was more generally read and admired in all parts of Europe than these letters. Dr. Franklin's principles bid fair to be handed down to posterity as expressive of the true principles of electricity, just as the Newtonian philosophy is of the true system of Nature in general."

The first letter in which Franklin alludes to the electrical power of *points*, and to the germ of the future lightning rod, is dated September 1st, 1747, and is as follows:—"In my last I informed you that in pursuing our electrical enquiries we had observed some particular phenomena, which we looked upon to be new, and of which I promised to give you some account, tho' I apprehended they might possibly not be new to you, as so many hands are daily employed in electrical experiments on your side of the water, some or other of which would probably hit on the same observations. The first is the wonderful effect of pointed bodies both in *drawing off* and in *throwing off* the electrical fire." [*The italics are Franklin's.*] He goes on to describe an experiment with an iron round shot of 3 or 4 inches in diameter (over which was suspended a small cork ball), and states that, if the shot be electrified, and then a long, slender, sharp, bodkin be presented to it, at some 6 or 8 inches distance, the electricity will be *drawn off* by the bodkin's point. He then says:—"To show that points will *throw off* as well as *draw off* the electrical fire, lay a long sharp needle upon the shot, and you cannot electrise the shot so as to make it repel the cork ball. Or fix a needle to the end of a suspended gun-barrel or iron rod, so as to point beyond it like a little bayonet, and while it remains there the gun-barrel or rod

cannot, by applying the tube at the other end, be electrised so as to give a spark, the fire constantly running out silently at the point."

In this letter we have the first intimation of the electrical power of points, and a clear proof that to Franklin is due the great honour of having been the first to introduce the knowledge of this power to the world. On this great fact will Franklin's fame for ever rest, and we shall endeavour to show that it was on this power *alone* that he originally based his invention of the lightning rod. That he actually *discovered* this power is, we are expressly told by Franklin himself, not the case. Dr. Priestley writes:—"Dr. Franklin, in the new edition of his Letters, says that the power of points to throw off the electric fire was communicated to him by his friend Mr. Thomas Hopkinson, who electrised an iron ball 3 or 4 inches in diameter with a needle fastened to it, expecting to draw a stronger spark from the point, as from a kind of focus, but was surprised to find little or none."—(Hist. Elect., 175.) But that Franklin was the original recogniser and employer of this property is acknowledged by Nollet, the eminent French philosopher, who opposed several of Franklin's views, and especially the merits of his lightning rods. According to Priestley, Nollet admits "that Dr. Franklin was the first who showed the property of pointed bodies in drawing off electricity more effectually and at greater distances than other bodies could do it."—(Hist. Elect., 165.)

The second letter from Franklin, in the sequence illustrating the development of his ideas on the connection of points and thunderbolts, is a memorandum headed "Properties and Effects of the Electrical Matter arising from Experiments and Observations made at Philadelphia, 1749," which was apparently sent to Mr. Collinson with the Letter IV. of July 29th, 1750. In this paper Franklin again explains the power of points to draw off as well as to throw off charge; and then he says:—"I say, if these things are so, may not the knowledge of this power of points be of use to mankind in preserving houses, churches, ships, &c., from the stroke of lightning, by directing us to fix, on the highest points of those edifices, upright rods of iron, made sharp as a needle and gilt to prevent rusting, and from the foot of those rods a wire down to the outside of the building into the ground, or down round one of the shrouds of a ship, and down her side till it reaches the water? Would not these pointed rods probably draw the electrical fire silently out of a cloud before it came nigh

enough to strike, and thereby secure us from that most sudden and terrible mischief?"

Dr. Priestley quotes another paper by Franklin, written on November 7th, 1749, or probably about the same time as the memorandum last mentioned was written; but this second paper does not appear in the third edition of the Letters from which we have been making extracts. After enumerating all the known points of resemblance between lightning and the electric spark, Franklin says, in this latter paper, "The electric fluid is attracted by points. We do not know whether this property be in lightning, but since they agree in all the particulars in which we can already compare them it is not improbable that they agree likewise in this. Let the experiment be made."—(Hist. Elect., 165.)

In these two extracts, and especially in the first memorandum, headed "Properties and Effects of the Electrical Matter," the invention of the lightning rod is clearly enunciated. They are written some two years after the discovery of the power of points was published; but it is evident that the invention of the rod was, in Franklin's mind, a corollary of the knowledge of this power. We see that all this time not one word is mentioned of any *conducting* power that such instruments might be supposed to have over the lightning or over the electrical discharge. In allusion to the invention of rods, and to the analogy that had previously been observed between lightning and the electric spark, Priestley says:—"It was Dr. Franklin who first proposed a method of verifying this hypothesis, entertaining the bold thought, as the Abbé Nollet expresses it, of bringing lightning from the heavens; of thinking that pointed iron rods, fixed in the air, when the atmosphere was loaded with lightning, might draw from it the matter of the thunderbolt, and discharge it without noise or danger into the immense body of the earth, where it would remain as it were absorbed."—(Hist. Elect., 164.) Here, again, there is no reference to the conduction theory,—*i.e.*, to the idea of conducting the discharge away from the building by means of the metallic stalk of the rod *after* the stroke or explosion has occurred.

Franklin's third letter in the literary chain showing the development of the lightning rod is dated October 19th, 1752, or nearly three years after the vital "Properties and Effects" memorandum. He gives an account in this third letter of an experiment made at Philadelphia (he does not say by whom) with a kite, consisting of a cross made of light strips of cedar with a silk handkerchief extended over

it, and having at the top "a very sharp pointed wire" rising a foot above the wood. The kite is held by a silk ribbon (in the hand) attached to the twine-cord, and the person must stand within a doorway, so that the silk does not get wet. "As soon as any of the thunder-clouds come over the kite the pointed wire will draw the electric fire from them," . . . "and when the rain has wet the kite and twine so that it can conduct the electric fire freely, you will find it stream out plentifully from the key on the approach of your knuckle."

It is to be borne in mind that Franklin, in his early letters, uses the term "electric fire" as signifying *electricity*, and not lightning, or the electric spark, as might perhaps have been imagined. In this third letter Franklin is evidently describing the famous experiment he himself made on July 4th, 1752. His language by no means justifies the inference that he conceived that he had drawn *lightning* from the clouds; and it is clear that his experiment was made solely to test his preconceived theory of the power of points. To all persons having a sound elementary conception of electricity it is abundantly evident that what Franklin effected in this experiment, and what his contemporaries—D'Alibard, De Romas, and Beccaria—effected with simpler apparatus (merely ordinary iron lightning rods with slight gaps in their length), was only to elicit manifestations, during thunder-storms, of the presence of Terrestrial Electricity, primarily in the earth below, secondarily and consequently in the thunder-clouds above. These manifestations were the sparks or flashes that passed across the air-gaps left in the apparatus. There was no explosion of the terrestrial condenser formed by the earth, the clouds, and the intervening atmospheric dielectric,—such as must happen when a *thunderbolt* occurs; and there was necessarily none of that luminous demonstration of the action of thunderbolts rightly known as lightning.

The fourth important communication from Franklin on these subjects is dated September, 1753, or about a year later. After referring to the question whether the direction of a thunderbolt discharge is usually descending or ascending, and coming to the conclusion that "'t is the earth that strikes into the clouds, and not the clouds that strike into the earth," he states that, in either case, "pointed rods fixed on buildings or masts of ships, and communicating with the earth or sea, must be of the same service in restoring the equilibrium silently between the earth and the clouds, *or in conducting a flash or stroke, if one should be*, so

as to save harmless the house or vessel." And, later on in the same letter, he says, "Metallic rods, therefore, of sufficient thickness, and extending from the highest part of an edifice to the ground, being of the best materials and complete conductors, will, I think, secure the building from damage, either by restoring the equilibrium so fast as to prevent a stroke, *or by conducting it in the substance of the rod as far as the rod goes*, so that there shall be no explosion but what is above its point, between that and the clouds."

Here, at last, we arrive (in the portions we have underlined) at the first mention by Franklin of the theory of the conducting action of rods. It is noteworthy that he still, in both passages, gives the preventive power of the rod's point the *first* place; but he now includes conduction of the explosion, after it has occurred, among the functions of the rod. The invention was now, however, public property. Chiefly through the influence of the great French philosopher Buffon, the *savants* of all civilised countries had by this time entertained, discussed, and investigated Franklin's views. He had been in communication with many of them; and we think there is very little doubt that it was principally owing to *their* immature ideas on the nature of electricity in general, and on the laws of its action, that this novel and unscientific theory, whereby the force evolved by a thunderbolt explosion is treated as if it were a current capable of conduction, was harboured by Franklin.

The fifth, and last, extract we shall make from the writings of this eminent man is dated June 29th, 1755, or nearly two years later on. He is writing to D'Alibard in regard to Père Beccaria's views, and he says—"As to the effect of points in drawing the electric matter from the clouds, and thereby securing buildings, &c., *which you say he seems to doubt*, I must own I think he only speaks modestly and judiciously. I have been but partly understood in that matter. I have mentioned it in several of my letters, and, except once, always in the alternative, *viz.*, that pointed rods erected on buildings, and communicating with the moist earth, would either prevent a stroke, or, if not prevented, would conduct it, so that the building should suffer no damage. Yet, whenever my opinion is examined in Europe, nothing is considered but the probability of those rods preventing a stroke or explosion, which is only part of the use I proposed for them; and the other part, their conducting a stroke which they may happen not to prevent, seems to be totally forgotten, though of equal importance and advantage."

The words we have underlined would seem to prove that

the leading European philosopher of the day was not at that time convinced of the power of points. In this last letter Franklin definitely entertains the conduction theory; but we think there is strong internal evidence in the letter tending to confirm the notion we have already expressed, that it was mainly in deference to European opinion that he made this addition to his original simple, but well-considered, idea of the efficacy of the rod lying solely in the power of its point in tending to prevent thunderbolt discharges,—in other words, in the action of the rod as a *tap* for the electricity of the ground and clouds. The conception of rods as *conductors* of explosions naturally gathered impetus from the fact of the inventor falling in with it; and this force necessarily received acceleration from the discoveries of the action of currents, obtained from chemical sources, made towards the commencement of the nineteenth century by Volta and his successors. The tendency (which has never yet been impeded) of imputing all electrical phenomena to the operation of *currents* became nearly universal; the term *electric fluid* was taken into general use, and *omne electricum ignotum pro currente* was (as it even now is) a motto of very extended practical application.

From the career of a physicist, Franklin gradually passed to that of a statesman; from the study of *matériel* he proceeded to that of *personnel*, and it is doubtful in which of the two spheres he showed the greater pre-eminence; but the latter was certainly the more cogent and engrossing, involving, as it eventually did, the foundation of a mighty Republic; and his diplomatic employment could have left him scanty time to reflect on, to improve, or to develop his great physical invention. It is interesting, however, to note that the French—the nation to whom the world principally owes the introduction and knowledge of Franklin's rods—have always paid more honour to his original views than have his own countrymen.

To begin with, the French recognise the preventive power of the rod's point by styling the apparatus a *paratonnerre*, whereas we English almost universally (and certainly in all official departments) call the rod a *lightning conductor*, being apparently unconscious that this expression is simply a contradiction in terms, inasmuch as it enunciates the idea of conducting an agency that can exist only in the absence of conducting elements. Indeed, strictly speaking, the idea is one of conducting merely the *manifestation* of an agency, and (so far as the writer can judge from a close analysis of upwards of six hundred detailed incidents of thunderbolt

action that have occurred in various parts of the world during the last two hundred years) not even the principal manifestation of that agency; and this makes the appellation still more absurd. In this connection it may be mentioned that the Americans—the successors, so to speak, of the great Philadelphian—invariably use his own term, *lightning rod*.

Again, both the French and the Americans employ on their buildings the original cheap metal that Franklin used and recommended for rods, *viz.*, iron; whereas in England the use of that most expensive metal, copper, is altogether the rule, and the employment of iron the exception. In this respect England is almost, if not entirely, alone among the nations of the world; and probably in no other country is the line of demarcation between electrical experts and tradesmen interested in the manufacture of expensive electrical apparatus so misty and vague. There are few lightning-rod manufacturers who do not consider themselves "electricians" as well, and do not utilise the status they assume as such to promote the sale of their copper wares.

Lastly, the French philosophers still generally adopt Franklin's theory of the dominant importance of points, and, in explaining the functions of rods, they give the preventive power of points the first place, and the supposed conducting action of the rod only the second. If, however, the English mention the preventive power at all, it is always given the second rank, and the conducting idea is awarded the place of honour. The lately departed and justly celebrated philosopher, James Clerk Maxwell, was, however, a unique and conspicuous exception to this prevailing English practice, for he unmistakably manifested his conviction that an ordinary lighting rod was simply a dangerous species of *electric tap*.

It is not within the scope of the present paper to enlarge on the theory of the action of lightning rods, or of the nature of terrestrial electricity,—nor to submit how, if the preventive action of a rod's point be admitted to be the sole advantage and *raison d'être* of the rod, the apparatus may be greatly simplified and cheapened. The writer's object is mainly to draw particular attention to the original ideas of his illustrious countryman, Benjamin Franklin, on the subject of his magnificent discovery; for we must remember (Transatlantic cousins please note) that Franklin was an *Englishman* when he pursued his remarkable physical career and introduced his beneficent invention. Let us Englishmen, then, honour our compatriot more in future in the

observance than as hitherto in the breach; and let us all, both Englishmen and Americans, besides noting his original physical ideas, note also his original human motives; for if we do so we shall assuredly find that his wish was not so much to protect the property of well-to-do Church bodies, of wealthy Manufacturers, or of Government establishments, as to defend the lives of the poor and the uneducated cottagers from the ravages made by thunderbolts in rural districts. Is it generally known that (as recorded by official returns) the number of persons killed by thunderbolts in Russia (not including Poland and Finland), in the five years from 1870 to 1874, was 2270, of whom no less than 2161 were dwellers in the country; and that during this period, in the same area, 4192 fires were occasioned by thunderbolts, 4099 of them being in the country?

Let educated men, interested in measures of defence from thunderbolts, obtain (as they easily can by reading) an *elementary* knowledge of the known laws of electricity, especially in regard to the actions of condensers, points, and metals; let them reflect on what they read; and let them *think for themselves* in regard to any steps they may deem it desirable to take for the defence of their own lives and property, or as a rampart for the lives and property of their dependents. Above all, let them follow the original intentions of Benjamin Franklin rather than the guidance of the "electricians" of the present day.

IV. HEATH'S BOLIVIAN EXPLORATIONS.

By Rev. J. D. Parker.

(Concluded from page 451.)

ON September 28th, 1880, Dr. Heath commenced his final voyage down the river. As he pushed his frail barque out upon the water, all the people gathered around and bade him adieu, firmly believing that he and his two Indian companions would be eaten by the cannibals supposed to be living along the Lower Beni. On October 7th, at meridian, he arrived at the lowest point reached on

his first descent, and stopped for the night at a point lower down, in 11° 4′ 46″ S. lat. The next morning at 8 o'clock he passed a stream coming in from the south, probably an outlet of a portion of the waters of Rojoaguado, an unexplored lake. To this river was given the name Ivon, in honour of the companion of Prof. Orton, in his last expedition to South America. At 10 o'clock he passed a point of rocks in the river, and came to an island with a large sand-bar in front. At this point he landed, and climbed one of the tallest trees to obtain a view of the country, as the dense forest cut off his view on every side. Far to the south, beyond the perennial forest that margins the river, he could see the glimmering waters of an unknown lake. At meridian he came to the junction of the Beni, with a large river flowing in from the north—undoubtedly the Madre de Dios. By triangulation he found the width of the Madre de Dios at the point of junction to be 2350 feet, and of the Beni to be 735 feet, the depth of water in the channel being 50 feet. All animals here were found to be exceedingly tame. He landed on an island, at the junction of the two rivers, within 10 feet of seven Cupibaras, which did not move, evidently never before having seen a man. The alligators found sleeping on the sand-bar did not stir, and the monkeys in the trees on the banks simply lifted up their heads while the boat passed under them, and then went to sleep again.

The only knowledge possessed of this river is the rumoured descent of it in 1860, by Señor Maldonado, a Peruvian colonel of engineers, who, being persecuted in his country for political reasons, is said to have descended the Madre de Dios and Lower Beni, losing his life at one of the lower rapids. Unfortunately all the notes of this expedition were lost.

Alligators had taken out of the boat every night, while moored at the shore, the meat of monkeys killed for provision, which rendered it necessary to shoot monkeys every day. Alligators here were very numerous, and, on account of their great familiarity, Dr. Heath shot two to make them respect them. For greater precaution he placed the meat at night at the head of his bed, as he slept on the shore near the boat, but in the morning the meat was gone, and the prints left by an alligator's feet and tail indicated by what means it was missing. He also found tracks of tigers in the sand near his sleeping-place, and indications where they had dug up turtle's eggs.

The Doctor pushed off his frail boat at 6 o'clock in the morning, and at 6.50 passed the mouth of a large river

flowing in from the north, to which he gave the name of Orton, in honour of the celebrated explorer. The land in this vicinity is low and marshy, with few trees. At midday he found a porpoise, which accompanied him all that day and a part of the next, until he came to the upper Rapids just above the Falls. The appearance of this porpoise gave him great encouragement, as the Madeira and Mamoré Rivers are full of porpoises, but the Beni has none. It was an indication that there were no dangerous Falls below him. During the day the Tabanos, a horse-fly, both yellow and black with white-tipped wings, disappeared, and myriads of sand-flies took their place. These sand-flies are found on all the tributaries of the Amazon, and Humboldt estimated that on the Orinoco there are a million of these flies to every cubic foot of air. There are two species of these sand-flies, called Pium and Maruim, differing only in size. During the night, while the Doctor was taking an observation with his sextant, the mosquitoes alighted on his face in such numbers that the blood trickled down on his garments. They got into his eyes, and alighted on his mercury, and disturbed his mercurial horizon to such an extent that he could not take his usual observation. The burning of the Pampas had so filled the atmosphere with smoke that he was compelled to work his artificial horizon without its usual glass cover, and stars of the first magnitude appeared as dim as those ordinarily of the fourth and fifth magnitude. The change from the Tabanos to the Maruims caused him to remember that the same change took place on the Mamoré just above the Falls, and he believed he was approaching the Falls corresponding to the same point in the Mamoré. In the distance he saw tapering columns of smoke escaping from the leafy roof of the forest, which probably curled up from fires burning at Indian camps beneath.

The next day the Doctor saw two turkeys on the bank, so tame that he shot one, and re-loaded and shot the other while it stood looking at its fallen companion. The same day he passed two large islands, and at 10 o'clock in the morning he reached a point where the river was obstructed by rocks, forming a rapid current, which he passed without difficulty, and soon afterward reached the main Falls. Failing to find a suitable passage for his boat on the north side, he passed over to the south side of the river, where he succeeded in drawing the boat over the rocks to the waters below. The difference of level between the water above and below the Falls is 28 feet, the angle of descent of the water in the main shoot is 25 degrees, and the falling water points

toward the southern shore, making a dangerous whirlpool, which was passed with great difficulty, his frail bark being almost swamped. The Doctor found a little bank below the Falls, where the two Indians managed to sleep while he sat in the boat all night writing and inking over his pencil notes made during the day, and occasionally bailing out the water from the boat, which had sprung a leak from the concussions received on the rocks the previous day. About 7 o'clock the next morning he passed a number of rocks in the river corresponding with the Rapids Palo-Grande of the Mamoré; for here he recognised the range of hills which he had seen while ascending the other river. Just below this point there is a large island, where Palacios spent the night in 1846, and the last point probably arrived at by Bursa, the Prussian explorer.

Dr. Heath now informed his Indian companions that they were near the termination of their voyage, as he recognised from the hills, and the island 4 miles below the Falls, that they had passed what corresponded to the fourth Falls in the Mamoré River. Ildifongo then asked if there was now hope for their lives? When informed that there was, he said he would call the falls Esperanza, or Hope. From this place they had an unmolested passage to the mouth of the Beni River, where they arrived October 11th, 1880. Here they found the *chacra* which the Doctor had made in August, 1879. There were over two hundred bunches of bananas, ripe and decaying, which furnished them an abundance of food. As the bananas had not been touched, the Doctor inferred the absence of Indians. Loading the boat nearly to the water's edge, they began the ascent of the Mamoré, and passed the first night at the foot of the first Rapids. But his Indian companions, having gorged themselves with food, were taken sick, and were unable to proceed for two days.

In ascending the Mamoré Dr. Heath met with a serious accident at the Falls of Palo Grande, where his boat was overturned and submerged in a fearful whirlpool, his cutting utensils lost, and all his provisions thrown into the river. The situation was critical in the extreme, and the Indians were stupefied with terror. Dr. Heath first secured his boat; then, seeing his *farinha* sewed up in an ox-hide, and two of his paddles floating around in the centre of the dangerous whirlpool, he plunged into the river. The Indians beheld this hazardous act with amazement, and uttered a cry of terror. Should the Doctor be drowned, how could they escape from the wilderness? and, if they should escape,

they would probably be shot under the suspicion of murdering him. Swimming to the *farinha* the Doctor placed his hand upon it, and, being carried round and round in the surging whirlpool, he gradually pushed it out; after which he secured two of his paddles in a similar manner, escaping from the whirling water after these almost superhuman efforts in a perfectly exhaustive condition. When the boat passed the Falls it was reloaded with the instruments which had been taken out and carried around. But the Doctor's paddle of green wood had sunk in the whirlpool, and he had lost all his tools for making another. Fortunately he found a piece of wood that had been roughly blocked out for a paddle, and by charring the wood and rubbing off the charred portion with a sharp stone he made himself another paddle. In this he displayed the same fertility of invention to supply his necessities which characterised the whole expedition. Their frail barque now leaked very badly, and the waves ran so high on the river during stormy weather that sometimes they were compelled to tie up to the shore. One night, while camping on the shore, the Doctor noticed that the boat began to fill with water, and he had barely time to arouse the Indians from sleep and unload the boat before it sank. On the following day, after much hard work, the boat was made to float, when it was reloaded, and they pursued their way.

On the 20th of October, while on the Brazilian side of the Mamoré, Dr. Heath heard some one calling, and saw with a field-glass a naked Chacobo Indian, who was standing on the sandy shore of the opposite side, and was trying to induce them to come across the river. When no attention was paid to him, twenty armed savages sprang out from the shelter of the trees, where they were lying in ambush ready to attack the boat should it approach that shore.

Without any further accident Dr. Heath arrived at Reyes December 11th, where he was received with every demonstration of joy. The school children, hearing of his safe return, went out from the town about 3 miles to greet him with songs and floral offerings. When he arrived at Reyes the church bells were rung, and there was a public ovation.

Soon afterward Dr. Heath ascended the Beni River on a balsa raft, and thus completed the map of the river from its mouth to its source near La Paz, when he took his departure from South America, and October 16th, 1881, he arrived in New York.

Most fruitful in results has been the exploration of the Beni River. It has materially corrected the geography of

that portion of South America. Two hundred leagues square of rubber trees have been opened to commerce, where each square league will contain from three hundred to five thousand trees. This will furnish work to large numbers of men ten months in the year, each man being able to collect from 10 to 25 pounds of rubber daily. It shortens the route of transportation of the products of the Lower Beni from 700 miles to less than 300, and necessitates the passage of only one fall in the river instead of five falls. For the larger portion of Bolivian commerce it obviates the toilsome transit over the Andes, and the long voyage around Cape Horn or through the Straits of Magellan.

V. ON TECHNICAL EDUCATION.

By Robert Galloway, M.R.I.A.

(Continued from page 412.)

It was stated in a previous article that the teacher in the Department's Science Schools, on account of being paid on the result system, must teach his pupils to pass. Whether they know the subject they are taught must necessarily be to the teacher, as far as his own self-interest is concerned, of very little, even of no, importance compared to that of their passing: he will therefore devote special attention to those who are most likely to pass,—to those who have, he finds, the best verbal memories. We will now give extracts from the reports of the Department's own examiners as proof of the correctness of this statement. But before giving the extracts, I will give a question and answer taken from the Report of the Select Committee on Schools of Art, published in 1864, on this subject. Mr. Edmund Potter, a member of the Committee, tendered himself for examination. Amongst other questions asked him, several related to what he would consider the best mode of paying teachers in the Department's schools in order to secure the best instruction. He recommended that the teachers should be paid by salary. He thus expressed himself as to the payment on result system. The question asked—"Do you prefer that system (payment by salary) to the system of

payment upon results?" Answer—"I think the system of payment upon results is a complete absurdity myself. I think it will have a bad effect upon the schools: a master will take the quick scholars and work them up, and the dull ones may take their chance."

In giving the following extracts from the Reports of the Department's own Examiners, I may state they are taken from the most favourable of their educational institutions, viz., the Training Colleges; and although, as far as I can make out from the 29th Report, only four annual examinations had been held up to December, 1881, in these Colleges, I think it will be admitted by all who read these extracts, that the science teaching in these institutions is of a very doubtful character.

It must not be forgotten or lost sight of in considering this part of the subject the length of time the Department has had the direction and complete control of the Governmental science teaching in the country, and the large amount of money Parliament has voted yearly for that purpose. If science teaching according to the *Department's system* had only been four years in operation, it would not be acting fairly to those having the direction and supervision of that system to expect even in the Training Colleges strictly efficient teaching in all the different branches of Science. But when we call to mind the fact that the Department of Science and Art has been in existence nearly thirty years the case is very materially altered. The Department was created in March, 1853; in 1855 the different Science and Art institutions of the country had been grouped under it, and the Parliamentary grant that year was £80,000. The grant increases every year. In the present financial year, 1883-84, it amounts to £365,690; the administration alone amounts to £32,110; and although the £365,690 is not all expended on schools and colleges, large sums have been expended upon them, so that if a sound educational system had been introduced, one more in accordance than the present one is with the systems adopted by other civilised countries, the nation would have benefitted, as Germany and other nations have benefitted, by the cultivation of the inductive sciences.

The following are the extracts I have alluded to: they are taken from the 29th Report of the Department:—

Prof. Judd and Mr. Lockyer, the Examiners in Physiography, say:—"The number of candidates in the subject of Physiography increased this year (1881) from 618 to 767. *Unfortunately* there is reason to fear that this increase was

caused by students being sent in for examination from colleges *where very inadequate instruction* is given in science. In many cases the answers show how imperfect the instruction has been. Thus *throughout whole colleges* we find the prevalent idea to be that *mercury in a centigrade thermometer cannot rise above* 100 *C., or sink below zero.* In other colleges *all* the students state that in order to *separate* a substance dissolved in water *from its solvent* it is necessary to *decompose the latter* by means of a voltameter.

Dr. Russell, the Examiner in Inorganic Chemistry, says: "I do not think that either the *theoretical* or *practical* examination was quite satisfactory. The *nature* of the answers and the *way* they are stated gave me the impression that *these subjects are learnt only with the object of passing the examination.*"

Mr. Thiselton Dyer, the Examiner in Botany, says:—"A detailed examination of the *successes* and *failures* still, however, leaves *much room for dissatisfaction.* With regard to *the method of teaching*, it is quite clear that the practice is still too largely resorted to of using *mere 'book-drill' instead of intelligent oral teaching.*"

Prof. Huxley and Dr. Michael Foster, the Examiners in Animal Physiology, say:—"There has *apparently* been a fair amount of diligence bestowed on the matter; this has been rewarded by a *large* number being *allowed* to pass. *But that diligence has been largely misdirected;* the *characters* of the answers *clearly show* that the candidates *prepare* themselves for this examination, *not by attempting to understand* physiology, *but by loading* their memories *with statements* which may be *re-produced* in the examination room."

Prof. Guthrie, the Examiner in Sound, Light and Heat, and Electricity and Magnetism, says:—"Generally, and in respect of both subjects,—sound, light, and heat, and magnetism and electricity—the answers have shown an improved, but *only slightly* improved, knowledge of physics; but in many cases *the teaching appears to have been mainly theoretical.* Many of the answers show that the examinee, while *understanding the conventional description* of a piece of apparatus, has *probably never seen it, and certainly never used it.* THIS WON'T DO."

The above extracts are instructive examples as to the *present state* of science teaching under the Department's system. It is difficult to credit, although stated by the Department's own Examiners, that the instructed in *many colleges* are so ignorant, not simply of science, but of common information, that they believe that mercury in a centigrade

thermometer cannot rise above 100° or sink below zero; or that the only way to separate a substance dissolved in water from its solvent is by decomposing the solven by a voltameter. Such dreadful ignorance is almost incredible; and the sooner a system, under which such ignorance fructifies, is swept away the better for the country; for, to use Prof. Guthrie's expression, "it won't do."

Practical as well as theoretical examinations in chemistry have been held during the last few years. 1878 appears to be the first year in which they were held. As I am about to cast a doubt upon the possibility of conducting the Honours Practical Examination in Organic Analysis, as given in the examination papers for Science Schools and Classes in 1882, in any Elementary Science School, it will be necessary to make some explanatory statements in order to carry my readers along with me. Neither the examiners nor the teachers are present at these examinations; they are in charge of persons who may have no knowledge of chemistry. How practical examinations can be conducted properly under such conditions I am at a loss to understand. In the Practical Examination in Inorganic Chemistry in the advanced stage, a substance is given the candidates, according to their examination papers, for "analysis qualitatively," which may contain any or all of thirty-four elements and compounds. But strange to say, in the higher, the Honours, paper the qualitative analysis is limited to a substance which will contain *only four* out of the thirty-four elements and compounds. Those who go in for Honours have in addition to make an estimation of the amount of lime, or some similar base, in a solution.

If the Examiners inspected these Science Schools, this practical instruction would certainly be a forward step in the right direction; but as long as inspection is not combined with the examinations, how the analytical courses are taught, whether by mechanical or scientific methods, cannot be determined by the Examiner; and how we teach, it must not be forgotten, is far more important than what we teach. I have already stated (p. 162) that a person totally ignorant of chemistry may be trained to perform mechanically different chemical operations, as a man can be taught mechanically to work a steam-engine. The porter in a chemical laboratory is frequently trained to prepare most difficult chemical preparations; and boys without the least acquaintance with the science are frequently taught in chemical factories to analyse quantitatively the substances manufactured in the particular factories to which they are attached. I have met

with—and I have no doubt other chemists have also—workmen in smelting works capable of assaying copper ores by the *dry method* with an accuracy which an experienced chemist could not equal until after some considerable practice. The mere performing the analysis, whether qualitative or quantitative, of inorganic substances, is no proof that the student who has performed them has been taught the science as a science. Dr. Russell, the Examiner, does not, as will be seen from the extract I have given from his report, believe that they are taught the science in the Training Colleges, but that "these subjects are learnt only with the object of passing the examination": in other words, it is mere cram.

The practical examinations in organic chemistry are far more difficult, and if the substance sent in the Advanced Stage for analysis is on a par as regards difficulty with what is too succinctly stated in the pamphlet entitled "Science Papers," I have no hesitation in stating that the student who analyses the mixture *correctly* is not likely to have been taught in a purely mechanical manner, for analytical tables will not enable him to analyse organic substances, as they do inorganic ones, altogether in that fashion; he must have a pretty good knowledge of the properties of the organic bodies he has to look for. I give what is contained in the printed document—

"ADVANCED STAGE."

"The examination in the Advanced Stage is confined to the qualitative analysis of a mixture which may contain any or all of the following constituents, together with one or two inorganic bases:—Hydrocyanic acid, formic acid, oxalic acid, acetic acid, benzoic acid, succinic acid, tartaric acid, and citric acid."

The time allowed for the analysis of a mixture of that sort, according to the document from which I extract it, is three hours; but in a later Time Table, which has been sent me indirectly from the Department, four hours is the time stated. Anyone who would analyse *correctly* in four hours a mixture containing 75 per cent of the organic acids named, and had to search for the whole of them, and that the two bases were of varied character, would deserve to be classed as an expert analyst: those unacquainted with chemical analyses would not comprehend the complexity of the analysis unless they were informed that it is frequently more difficult to prove the absence correctly of a substance, especially in organic mixtures, than to prove correctly the

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presence of one, if the mixture is at all a complicated one.

Before quoting the Honours paper we must recall to the recollection of our readers the class of students on whom alone the teacher can receive payment on results, the *time* when he must teach them in order to be able to receive the payment if they pass the examination successfully, and other particulars; but the question about the numbers who come up for examination and the cost must be deferred until the next article.

The teacher receives payment on results of instruction as tested by the examination of the students; only for those who belong to the artizan class, or persons, and their children, who are in receipt of not more than £200 a year, and teachers and pupil teachers in some elementary schools. The time for the class to meet according to the Department's instructions, in order to receive the payment on results, is after 6 p.m., every week-day except Saturday; on the latter day it can meet at 2 p.m. Twenty-eight lessons must at least be given in each subject; when the subject is taught theoretically it must last at least one hour; but when it is taught practically it must last at least one hour and a half; and two lessons on the same subject cannot be given on the same evening or afternoon. All the subjects are divided into three stages, and a separate set of examinations is set in each. In each stage—Elementary, Advanced, and Honours—there are two grades of success, 1st and 2nd class. "The Elementary and Advanced Stages," states the Science Directory, "are for the ordinary Science Schools. The Honours Examination is of a very advanced character." It will be observed that in the Directory the Department is very mysterious about the Honours Examinations; they are equally mysterious, as we shall find further on, as to the number who go in for them in each subject; they do not say, it will be observed, whether the examinations in Honours *are held or not* in the night classes. If they are not held in these classes, why put down the payment on results which the teacher will receive for this stage, because on no other class but the artizan, and £200 a year, class, can the teacher receive result payments, and these classes must, we have seen, be taught at night to receive even on them.

The payments are as follows:—

1. In the Elementary and the Advanced stages of each subject, except Practical Chemistry, £2 and £1 for a first or second class respectively.

2. In Honours, £4 and £2 for a first or second class respectively.
3. In Practical Inorganic and in Practical Organic Chemistry, £1 10s. and £1 for a first or second class respectively in the Elementary stage; and in Practical Inorganic and in Practical Organic Chemistry, £4 and £3 for the first or second class respectively in the Advanced stage and in Honours.

It is noteworthy that the payments are the same in amount in the Elementary and Advanced stages for the different subjects, including chemistry, when taught *theoretically*, but it is reversed when chemistry is taught *practically*, the payments made in the Advanced stage being equal to those made in the Honours stage.

I am fearless in avowing my disbelief that the examination in Organic Analysis in the Honours stage takes place in any of the Elementary Science Schools; for even in such a pronounced scientific institution as the Victoria University *only a few* students could be examined at the *same time* in that subject. I will first give the examination paper as given in the "Science papers," and then I will state my reasons for the opinion just expressed.

"HONOURS."

"The examination in honours consists of a qualitative analysis of a mixture supplied, and of the determination of the percentage of carbon and hydrogen contained in the organic substance distributed for this purpose.

"The bottle marked (A) contains a substance in which the percentage of carbon, hydrogen, and oxygen (the latter by loss) is to be determined by ORGANIC ANALYSIS."

For the information of those who are not acquainted with chemical analysis, it is desirable to state that, for the mere execution of the analysis, no great amount of chemical knowledge is required; but great manipulative skill is requisite for its successful execution; also costly apparatus is required; and a *space*, varying in extent with the number of persons who can be making the analysis at the same time, but considerable in extent, even for one person, must be specially fitted up in the laboratory for that analytical operation alone. It is also a very lengthy operation as regards the time required, even for one analysis, and, unlike most other analytical operations, it must be completed on the day it is commenced.

XUM

The time allotted for its execution at the examination is, according to the Department's time table, from 2 p.m. to 10 p.m., which is not too long; but if it requires that length of time in the examination room to perform it successfully, it must require for its proper execution an equal length of time in the class room, so that those who commenced to make an organic analysis at 6 p.m. in the night class would have to continue at their labours until about 2 a.m. the following morning. Further, each of the candidates at the examination, as they must be started equally, must be provided with a *balance* and a box of chemical weights, and the balance, as accuracy must be the test as to the proper execution of the analysis, must be so delicate that it will *turn* with the one-thousandth part of a grain. Other costly apparatus would also be required by each student, and, as has been stated, a large space must be specially fitted up in the laboratory for the purpose.

What science teacher who is paid on the result system would undertake to instruct students, even if he were capable and had the necessary apparatus, &c., in this difficult branch of chemical analysis; for if they gained a second class only, he would only receive a third more in money than for a first in the theoretical elementary stage, and he could *cram up* 20, 30, or 40 students for examination in the latter stage in the time it would take him to teach one or two students to make organic analysis, and in the elementary stage he could count on a larger percentage of successful students.

But how are the laboratories for these night classes fitted up, even for the study of practical inorganic chemistry in the most elementary stage; I will give the description of one, as it has been given to me, in one of the leading manufacturing towns in England. It was sent me by a gentleman who is personally unknown to me. He wrote to ask me for advice on educational matters. I advised him to study chemistry in a laboratory: the only time he could devote to study was the evening. He therefore went to the Practical Class under the Department, but he gave up attending it after a short time owing to the disorder preventing any work being done. This is his description:—

"The laboratory class I attended for a short time was under the Science and Art Department; it was at the Central Board School. The laboratory was originally intended for the day scholars. There seemed to be no kind of order kept with the day pupils, for when I went in the evening I had to *hunt* for most of the test bottles, and even our private

cupboards were *rifled* of necessary apparatus. You will readily conceive *how much work* it was possible to get through during the hour and a half, when *most of the necessaries were to be sought for and cleaned or filled ere anything could be done.* It was not in the teachers' power to mend matters, as he had nothing to do with the day class." That laboratory certainly was not a favourable place for studying even the rudiments of inorganic chemistry, and therefore certainly not for learning organic analysis.

With respect to the number of places fitted up for organic analysis in laboratories which are almost exclusively devoted to research in organic chemistry, I may state that a friend of mine informed me only the other day that when he was studying in the leading laboratory in Paris, there were *only two* places. Let me add, further, that evening classes are carried on in the Victoria University, Manchester; in looking over their Examination Papers for the Session 1877-78 (I have not a later Prospectus to refer to), I find *no such* examinations as those the Department state they carry out in their schools. From the experience I have had in conducting and superintending practical examinations in analytical chemistry, I consider that it would not only be desirable, but that it is absolutely necessary, for conducting them properly that these examinations should be in charge of a chemist who has had experience in teaching that branch of the Science. Even for the Honours stage in inorganic chemistry, scientific supervision is required to direct the examinees in the arranging of their work, so that they do not clash by each wanting, for instance, the same piece of apparatus at the same time.

Even if the practical organic examinations could be carried out, there nevertheless arrives a time when it is most desirable that examinations should not be prolonged, whatever may be the subject examined upon, and whatever may be the object of the examination. Mr. Todhunter, in his work "The Conflict of Studies," and other acknowledged authorities, strongly insist upon this point. But under the payment on result system, even for the teacher to earn the sum of £1, the costly and inefficient examination machinery of the Department has to be set in operation; the plant of knowledge is thus being constantly plucked up to see how it is growing; the authors of the system forgetting or disregarding that "study requires some amount of peace and quietness, without the constant expectation of being called to 'stand and deliver.'" Would that our senators at least would ponder over the following pregnant remarks by Mr. Herbert

Spencer :—"And pervading the whole, is the vicious system of rote learning—a system of sacrificing the spirit to the letter. See the results. What with perceptions unnaturally dulled by early thwarting, and a coerced attention to books, —what with the mental confusion produced by teaching subjects before they can be understood, ... —what with making the pupil a *mere passive recipient of other's ideas, and not in the least leading him to be an active inquirer or self-instructor*, —and what with taxing the faculties to excess—there are very few minds that become as efficient as they might be. Examinations once passed, books are laid aside; *the greater part of what has been acquired, being unorganised, soon drops out of recollection; what remains is mostly inert, the art of applying knowledge not having been cultivated, and there is but little power either of accurate observation or independent thinking.* To all which, add that, while much of the information gained is of relatively small value, an immense mass of information of transcendent value is entirely passed over."

If, in order to relieve our Senators of embarrassments in connection with Civil Service appointments, competitive examinations have been resorted to as the only possible means of selecting candidates for these appointments, why employ them at least to the extent that is at present being done, to test the extent and progress of knowledge in educational institutions, when it is admitted by all competent authorities that the general education of the kingdom is being seriously injured by these competitions?

Written examinations are no test of sound attainments in any branch of knowledge; but they are the least fitted to distinguish thorough from superficial knowledge in the inductive sciences. The only thing these examinations do test is the relative capacity of the candidates for acquiring and retaining for a time the ideas of others, and of reproducing them on paper. This is a very desirable quality for those intended for the legal profession to possess; but it is a quality almost the reverse of desirable for those who study the inductive sciences either for their purely scientific extension or for their technical applications. This examination system is entirely opposed to the educational system carried out in Germany: the German students are taught the art of applying knowledge and of extending it; our students are merely taught to be passive recipients of others' ideas, and to be capable of reproducing them on paper. See the results. German chemists are year by year being more employed in our chemical factories, so that, since the time the Science and Art Department was created, our calico-

printers have had to rely more than they did before its creation upon the Paris *ateliers* for designs; and our chemical manufacturers seek far more than formerly scientific aid from Germany.

Is this a pleasant picture for Englishmen to contemplate after all the heavy taxation they have had to endure for thirty years for the promotion, as they have been led to believe, of scientific and technical instruction? What reply can that most expensive and overgrown bureaucratic Department make to this statement of facts?

NOTE.—Even if organic analysis could be carried out in the Elementary Science Schools in conformity with the Honours' paper, it would nevertheless be a most inappropriate competitive test as to the relative degrees of skill and knowledge of different students in these, or in any, schools or colleges. Because the *glass tubes* in which the analysis has to be made will not unfrequently break on the application of the required heat; and if the tube breaks the analysis is destroyed, and has to be commenced afresh; and the breakage most frequently occurs from no want of skill on the part of the analyst. If, then, for example, out of six students competing in this stage, the *tubes* of three of the students were to break, how could marks be fairly given at all in such a case.

(To be continued.)

ANALYSES OF BOOKS.

*Wieder die Humanasters! Rechtfertigung eines Vivisectors.** By FRIEDRICH GOLTZ, Professor in the University of Strassburg. Strassburg: Karl J. Trübner.

FOR this incisive and decisive little work we must bespeak the careful attention of our readers, and of all persons who are desirous of forming an honest opinion on the so-called "Anti-Vivisection" question. A word may be needed concerning the title. As the poetaster is a caricature of the true poet, and the philosophaster of the genuine philosopher, so the humanaster is a burlesque on the man of rational humanity. This word, proposed by the Rev. Henri Tollin, of Magdeburg, is no less happy than Prof. Owen's term "Bestiarian."

Turning from the title to the subject, we must introduce the reader to a man and a book. The book is the "Torture-Chambers of Science,"† the official manual of German bestiarians. The man is its author, a certain Ernst von Weber, a "knight of high orders," as he seldom forgets to inform the public. To get an idea of this man we must imagine our leading British bestiarians all blended together into a "thick and slab" compound.

Now Prof. Goltz has admittedly performed a number of experiments upon dogs, chiefly with a view to elucidate the organisation of the brain and its relations to the entire nervous system. He has described these experiments and their results in a special work. In the Introduction to this work he has expressly stated that all the subjects upon which he operated were, before the commencement of the experiment, brought profoundly under the influence of chloroform, and were hence absolutely unconscious of pain.

The "Knight of high orders," however, in compiling his "Torture-Chambers," has quoted numerous passages from the work of Prof. Goltz, as proofs of the cruelty of physiologists, and these quotations have naturally produced a sensation of horror among the lay public. But our "Knight" has carefully concealed the fact that the animals had been subjected to an anæsthetic, leading, or at least allowing, his readers to suppose that they were in their normally sensitive state. Herr *Von* Weber has therefore here been guilty of the *suppressio veri* in a manner which would have been discreditable even to a pettifogger or a professional agitator. But he has gone still further; in two passages of his "Torture-Chambers" (edit. 5th, pp. 11 and 26)

* Against the Humanasters! Justification of a Vivisectionist.
† Die Folter-Kammern der Wissenschaft.

he has formally asserted that Prof. Goltz did not make use of anæsthetics! Now as he has carefully studied the writings of Prof. Goltz, in order to extract from them whatever passages, duly garbled, would serve his purpose, he has in these passages wilfully and consciously stated the "thing that is not." It is not our custom to use, in the pages of the "Journal of Science," those plain old Saxon words which can alone fitly characterise this "Knight" and his style of controversy and agitation; but we leave our readers to draw their own conclusions as to the weapons which the Bestiarian advocates think worthy of themselves and their cause, and as to the materials upon which Anti-vivisectionist sentiment is founded.

Herr Von Weber's assertions have been repeated over and over again by his imitators and followers, and the public—good, easy souls!—have never looked into the writings of Professor Goltz to see if Herr Von Weber's quotations convey the truth, the whole truth, and nothing but the truth.

Surely this one case on the part of one who is, out of England, probably the most prominent "Humanaster" in the world, ought to inspire all candid inquirers with a wholesome caution. Well and truly says Prof. Goltz, "The humanaster is an inversion of the rag-collector. The latter seeks out from amidst rubbish whatever is capable of utilisation. The humanaster grubs in dirt for the sake of the dirt. Whatever seems fit to excite horror and disgust is eagerly dragged forth; whatever is useful to mankind is carefully hidden, so that the multitude may know nothing about it."

Prof. Goltz then proceeds to convict the Bestiarians of want of truthfulness in other directions. They claimed Darwin as one of their adherents, till his letter to Prof. Holmgren and his formal accession to the Society for the Promotion of Medicine by Research refuted the calumny. In like manner the enemy have claimed Cuvier, Sir Charles Bell, and Hyrtl as authorities in their favour! Prof. Goltz refers his readers, on the contrary, to Cuvier's *éloge* of the vivisector Flourens. The assertion that Bell rejected experimentation on living animals he proves to be a pure invention, and cites numerous passages from the German version of the works of the great anatomist. Hyrtl, he shows, was diligent in vivisection,—often unnecessarily so.

Having thus disposed of the real men of Science whom the Bestiarians falsely claim, the author declines to notice the "scientific walking gentlemen" who swell the ranks of the enemy. If, as the Humanasters allege, all the problems of physiology can be solved without the aid of vivisection, he challenges them to prove their words.

With an able sketch of the inconsistencies of the Bestiarians, and of the shifts and shuffles with which they seek to justify or to palliate such inconsistency, he concludes his little but weighty work.

We would venture to recommend this pamphlet to the careful notice of all biologists and physicians. Would not the Society for the Promotion of Medicine by Research find a translation of Prof. Goltz's plain, straightforward publication an excellent work for distribution?

Micro-Photography, including a Description of the Wet Collodion and Gelatino-Bromide Processes, together with the Best Methods of Mounting and Preparing Microscopic Objects for Micro-Photography. By A. Cowley Malley, B.A., M.B., B.Ch., T.C.D. London: H. K. Lewis.

Without Micro-Photography microscopic research is deprived of one of its most important auxiliaries. The observer may, indeed, preserve his mounted objects to refresh his memory, or for exhibition to his friends and colleagues; but if he wishes to lay his results before the scientific world at large, he, but for micro-photography, has no other resource save drawings executed by hand. Concerning these Dr. Malley very justly remarks:—"I have before me at present some drawings which must have taken the artist a week to finish, and even then show the existence in his mind of preconceived notions of structure."

The second chapter of the little work before us is devoted to the properties of lenses, the imperfection of resulting images, aberration, &c. Though much of the information here given will doubtless be familiar to all persons likely to be working with the microscope, the views of Prof. Abbe, of Jena, here quoted, may be of advantage to many.

In the third chapter we have an account of the microscope, and of the means of illumination, for which purpose the author recommends Swan's electric lamp. The camera and its accessories, the arrangement of the dark room, and the mounting and preparation of objects for micro-photographic purposes are next described. The photographic processes are next considered.

The receipts for the solutions required in the collodion process are given in metric weights and measures, though in the developing solution we meet with the unusual abridgment in alcohol, 2 ℥. The formula for the hyposulphite bath is given, however, in ounces, the pharmaceutical symbol being somewhat strangely employed. On pp. 83 to 88 the formulæ employed are all expressed in pharmaceutical symbols. This is surely an error, since all those of Dr. Malley's readers who are neither medical practitioners nor pharmaceutists will fail to understand these somewhat cabalistic characters, and before setting to work will have to get them interpreted. In our opinion all the formulæ

should have been given either on the metric system or else in "parts." Passing over this survival of the unfittest, the work seems likely to be of great service to microscopists.

Elementary Text-Book of Physics. By J. D. EVERETT, M.A., D.C.L., F.R.S.E., Professor of Natural Philosophy in the Queen's College, Belfast. London: Blackie and Son.

AFTER a careful examination we must pronounce this work—as might, indeed, be naturally expected from the standing of its author—unexceptionable, both in the matter and the manner of its teachings. It may seem, therefore, paradoxical if we declare ourselves unable to accept certain views here laid down. Dr. Everett, in his Preface, writes:—"It [*i.e.*, the book before us] aims at presenting, in brief space, those portions of Theoretical Physics which are most essential as a foundation for subsequent advances, while at the same time most fitted for exercising the learner in logical and consecutive thought. It does not give minute directions for manipulation; but, avoiding details as much as possible, presents a connected outline of the main points of theory.

"In order to place Science upon an equal footing with the more established studies of ancient languages and mathematics, as a means of practically training the bulk of our youth to vigorous thought, it seems necessary that Science text-books should be constructed upon such lines as these. It is not practicable to make the bulk of the boys in our public schools expert scientific manipulators, but it is practicable to ground them well in the main lines of scientific theory. The aim must be not so much to teach them many facts as to teach them rightly to connect a few great facts together. Science must be taught them from a liberal, not from a technical, standpoint."

We regret that we cannot accept the author's contention. We submit that the object of the introduction of Science into an educational course is to teach youth not mere "vigorous thought," but a something which classics and mathematics cannot even attempt to do, and which they rather unteach when cultivated alone, *viz.*, the arts of observation, and of drawing right conclusions from the phenomena witnessed. But this cannot be done by reading or by hearing lectures, or even by witnessing experiments. An eminent American authority has said that the only sensible way to teach Botany is to begin with plants, and merely refer to books by way of summarising the facts thus learned. Science should be taught the pupil not from a *wordy*, but from a *thingy* standpoint. The exact degree of manipulative

skill acquired is not so much the question as the acquisition of those habits of observation which our present and past systems of education have aimed at destroying.

The work before us would furnish an admirable text-book for pupils who can in every case start from the facts.

Household Chemistry: for the Non-Chemical. By ALFRED J. SHILTON, F.C.S., Lecturer on Organic Chemistry at the Midland Institute, Birmingham; and at the Walsall Institute. London: F. V. White and Co.

WE have here a work which, though in smaller compass, reminds us in some respects of Johnstone's "Chemistry of Common Life." It differs, however, in the more copious introduction of chemical formulæ.

The author remarks that certain earlier works, covering to some extent the same ground, have been at fault in containing matter "not of a strictly chemical nature, and which, however interesting in itself, swells the book to a large size without adding to its usefulness." On this we may remark that in a popular treatise it is sometimes difficult to observe the boundary-lines of strict method. Of this Mr. Shilton himself is an instance, since in his chapter on Candles (Chapter IV.) a great part of the information given relates to purely mechanical operations. No small portion of the concluding chapter, also, might be referred to a treatise on "Household Physiology."

The instruction given in the work must be pronounced, as far as we perceive, accurate in itself and clear in exposition. We are much pleased with the author's remarks upon ozone, the faith in which may deservedly rank as one of the superstitions of this scientific age. We are far from certain whether, when, and to what extent, this body exists in the atmosphere. Granting its presence, its sanitary value is a still more doubtful question. From experiments made with ozone, as artificially prepared, it seems to have no very beneficial action upon animal life.

The chapter on Water also contains much of which we must express our approval. The author gives it as his opinion that soft waters are dietetically preferable to such as are hard. With this we agree. That some towns supplied with soft waters have a higher death-rate than that of certain others consuming hard waters proves nothing, unless all the other conditions were alike. We scarcely agree, however, with the author's expressed preference for the waters of deep wells as against that collected from uncultivated siliceous uplands. We fear that in populous

districts organic pollution must gradually extend deeper and deeper into the earth's crust.

The author's condemnation of the use of plaster of Paris in the sophistication of wine is very much needed. Were the Customs instructed to confiscate and destroy all wines containing more sulphuric acid than the maximum found in the juice of the grape, this mal-practice would soon come to an end.

Mr. Shilton's work may be safely recommended.

Economy of Coal in House Fires, or how to convert an Ordinary Fire-Grate into a Slow-Combustion Stove at a Small Cost. By T. PRIDGIN TEALE, M.A., F.R.C.S. London: J. and A. Churchill. Leeds: C. Goodall.

THE author's principle is to cut off the current of air which ordinarily passes through the fire-grate and its contents from below, and to keep the space beneath the grate hot. It is admitted that the mere interception of the bottom draught without the warm chamber beneath the fire-box will be unsuccessful. With his first rule we agree most heartily:—"As much fire-brick and as little iron as possible." It is to be regretted that for the last half-century we have been retrograding in the conconstruction of fire-grates. The object of inventors in this direction—mostly founders or ironmongers—has been to sell a maximum weight of cast-metal. With the exception of Mr. Teale's "economiser," a sheet-iron shield for shutting in the chamber under the fire, he returns in most points to the structure of the grates used a century ago. He recommends a horizontal fire, deep from front to back, and with its back and sides of fire-brick.

We have no doubt that the arrangement here described is a step in the right direction. Nevertheless we think that comfort, economy, and the necessity of minimising the products of combustion in our towns, all bid us to go still further, and renounce our open fires in favour of stoves constructed of tiles and fired up in the passage.

XUM

Report of an Exploration of Parts of Wyoming, Idaho, and Montana, in August and September, 1882; made by Lieut.-Gen. P. H. SHERIDAN. With the Itinerary of Col. J. F. GREGORY; and a Geological and Botanical Report, by Surgeon W. H. FORWOOD. Washington: Government Printing Office.

THE first fact we notice in the topographical account of the region traversed is that the trout of Spring Creek and Wind River have dark spots on their sides, whilst the trout in the streams east of the Mississippi have red spots. It is pleasing to learn that Gen. Sheridan prohibited the killing of more antelopes than the Expedition could consume.

It is remarked that the water of the Shostone Lake is "as blue as indigo." Whether this colour, as in some small lakes in Transylvania, is due to a salt of copper, is not mentioned.

Gen. Sheridan notices that the National Park is in some danger from encroachments, and that the game is being rapidly killed off. He proposes to keep out the skin-hunters by means of his troops. Col. Gregory also notices acts of "vandalism," from which the magnificent geysers of the park have suffered no little.

The speed at which the Expedition moved was inimical to geological and botanical research. Nevertheless Dr. Forwood made a considerable number of interesting observations. He finds, by comparison with the reports of former visitors, that the great geysers are not decreasing perceptibly in strength.

It is curious to notice the paucity of ferns encountered, the only species recorded being *Woodsia Oregana*.

Second Report of the Department of Entomology of the Cornell University Experiment Station. By J. H. COMSTOCK. Ithaca: Andrus and Church.

THIS Report deals exclusively with what may be called the economical phase of Entomology,—the identification of the insects harmful to field- and garden-crops, the descriptions of their habits, and suggestions for their extirpation, or at least diminution. This study has been forced upon the agricultural interest in the United States in consequence of the exuberance of insect-life under their more brilliant suns; partly also, perhaps, from the large size of the fields, and the substitution of rail- and wire-fences for live hedges, thus depriving the insectivorous birds of shelter. Hence in America the entomologist is no longer sneered

at as he once was, and as he still is in some parts of "this our highly favoured country."

It comprises a valuable monograph of the scale-insects,—pests which are now being transmitted from one country to another, and which, even where the climate is uncongenial, make themselves at home in conservatories.

From this Report we may learn how easy it is for insects to change their diet, and thus suddenly rank as enemies to the husbandman.

Another reflection, not to be overlooked by Zoophilists, Neo-Buddhists, and the like, is the impossibility of existing without taking life. If we eschew all animal food we must still destroy animals by myriads, or no vegetable nutriment will be left for us to consume.

Memoirs of the Geological Survey of India. Vol. XIX., Part 1. Calcutta: Geological Survey Office. London: Trübner and Co.

THIS part is an account of the Cochar earthquake of January 10th, 1869, by the late Dr. T. Oldham, F.R.S. At the time of the occurrence he was at Barrackpore, about 15 miles from Calcutta, but, according to the instructions of the Lieutenant-Governor, he went to Cachar and Silchar to observe the results of the shock at its centre. It appears that six or eight shocks had occurred in 1866, chiefly between January and April, whilst none are recorded in 1867 and 1868. It is remarked that in the Silchar cemetery the upper slab of a tomb, a heavy mass of marble, had been projected from the body of the tomb more than 8 feet in a north-north-easterly direction.

At Sylhet, according to the Executive Engineer, a peculiar tremulous, "almost stinging," sensation was experienced in the feet. At Manipur it was noted that, though the shock lasted about a minute and a half, the lower animals did not seem at all affected.

Passing over numerous details of the effects of the shocks in different places, we come to generalities.

It is concluded that the shock originated in a fissure about 20 miles long, on the northern margins of the Jaintra Hills. The mean depth of the focus is calculated at 30 to 35 miles, and the velocity of motion of the wave particles as high as 30 feet per second. This being double that observed by Mr. Mallet in the great Neapolitan earthquake of 1857, the question arises why the loss of life and destruction of houses was so much smaller in India than in Italy? The cause must be sought in the

different character of the architecture. In India the majority of houses are elastic structures of wood and bamboo. Those built of brick or stone are for the most part only one storey in height, and detached from each other.

We cannot avoid here repeating an old lesson, *i.e.*, insisting on the advantages in districts liable to earthquakes, of houses built of sheet-iron plates bolted together. To prevent rapid changes of temperature the walls may consist of double plates, with a layer of air between. Such houses would have, in addition, decided advantages in a sanitary point of view, and would not harbour snakes, rats, termites, and vermin in general.

Breeding Horses for Use. By F. Ram. London: Civil Service Printing and Publishing Company, Limited.

We felt at first slightly surprised that a book on such a subject should be sent to us for review. On turning over its pages we soon perceived that the author was taking up the matter from a novel, and decidedly rational and scientific, point of view. Mr. Ram's object seems to us, indeed, of no small importance. He seeks "to point out the irrationality of the present means of determining which particular horses are best to be employed in propagating their kind,—that is to say, for raising stock for useful purposes." In view of a variety of circumstances both at home and abroad, he contends that it is our duty to obtain from the farm-produce placed in the stable-manger the maximum amount of power obtainable from it. He shows that the amount of work to be got out of horses for an equal consumption of food varies. The object of rational horse-breeding is to obtain "from the same number of blades of grass or grains of corn as now yield only one foot-ton of work, a much larger measure of energy." He holds that horses should be judged not by their figure, or their power of "flying, lightly weighted, as fast as scandal, for a few moments, for the benefit of gamblers," but for their capacity for work, their power of endurance, and of bearing great fatigue, privations, and hardships: the horse which excels in these respects will be the best, whether for the road, the farm, or the battle-field, even if he should display what some authorities call "inferiority of figure and utter want of quality." He says, "How use and beauty may be in conflict is illustrated in the Californian mustang, described in the "American Journal" as a coarse-grained, ill-built, ugly, vicious brute, but invaluable on account of his never-to-be-played-out bottom and speed. With what favour would such an ugly beast be looked upon if it made its appearance at an English show?" How comes it, we may ask, that prizes have

never been offered for the horse which can cover most ground in a day, or in a week? The answer, we fear, must be sought in the fact that such races would ill suit the class to whom the horse is a mere tool for gambling.

Mr. Ram's work deserves, from a national point of view, very careful consideration.

Remarks on Cattle-Plague Vaccination. Translated from the French of Dr. C. PIGEON, of Fourchambault. With an Appendix on the Inoculation of Goats with the Virus of Pleuro-Pneumonia. London: Vacher and Sons.

A FEW days ago the French nation, by their Parliamentary representatives, have voted almost unanimously to Prof. Pasteur a pension of 25,000 francs annually, with remainder to his wife and children. All the political parties seemed agreed, and the discussion raised by a few members was barely tolerated. In full opposition to this vote, Dr. Pigeon proclaims Pasteur's discovery a complete failure! Surely such a question as that of the efficacy or inefficacy, the safety or the danger, of a method of treating a given disease admits of being brought to an absolute decision! On the title-page of the pamphlet is given an extract from a speech of Dr. B. W. Richardson, F.R.S., in which we find these words:—"This manufacture of spic-and-span new diseases in our species is too much to endure the thought of, especially when we know that purity of life is all-sufficient to remove what exists, without invoking what is not." Very fine; but what definite, practical meaning are we to attach to the floscule "purity of life"? Taking the words in their sanitary meaning, we cannot forget that rinder-pest sometimes passed over cattle lodged in filthy, undrained, and unventilated stalls, to attack a herd in some model farm where every conceivable appliance was used to secure the absence of all foci of disease. We should like, therefore, some tangible instructions. As for the goats in South Africa, the sooner they are extirpated the better.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

NATURAL LAW IN THE SPIRITUAL WORLD.

To the Editor of the Journal of Science.

SIR,—It has remained for a Scotch theologian, Henry Drummond, F.R.S.E., in a book with this title, published by Hodder and Stoughton, to show, to his own entire satisfaction, that the discovery of unity in Nature limits the members of the kingdom of Christ to an astonishly select few. It is a pitiable work, not worth notice except that the title may in some few cases lead to waste of time.

To correspond with the spiritual environment, the author says, it is needful to die to the natural environment. The shortest way out of the difficulty (p. 180) would be to die at once and go to heaven; but, as this is not permitted, we must "die as much as we can," the task being limited to three steps—"Suicide, Mortification, Limitation." The author, who seems to be the impersonation of relentless bigotry, clutches, with an energy which is fearful to see, at a floating straw in the shape of a supposed analogy between Nature and some of St. Paul's words, which, as we know, are capable of being wrested to a purpose too sad to be dwelt upon. We are reminded of a Scotch dame who used to deliver herself thus:—"The true Kirk o' Campbell, as compared with the outer warld, is compact of my gude man and mysel', and I'm na that sure o' John."—I am, &c.,

H. H. H.

PS.—Can it be possible that we have here another specimen of a pious fraud, meant to frighten the Evolutionists? From a letter in the "Spectator" it would seem that the author of "Ecce Homo" had something of this kind of purpose in his more recent work on Natural Religion. Does Mr. Drummond believe his divinity, but not his science? Well, this would certainly account for some rather peculiar views that Mr.

Drummond has, notwithstanding his being F.G.S. in scientific matters.

OFFICIAL AND NON-OFFICIAL WATER EXAMINATIONS.

To the Editor of the Journal of Science.

SIR,—Premising that I have not the slightest respect for Water Companies, either in London or elsewhere, but look upon their establishment as one of the most serious errors of modern times, I must yet protest against the outcry raised in certain quarters concerning the analytical reports published on their behalf. It is, indeed, not easy to see who is the party really aggrieved, or what there is to complain of. Nobody, I presume, will doubt the competence of Mr. Crookes, and of Drs. Odling and Tidy, even if their results do not coincide with those of the official examiner. Nobody can deny that the Water Companies, if so disposed, have the right, like any private individual, to have their wares analysed, and to publish the results. Nobody, surely, except amiable enough to believe in the infallibility of the official examiner and the all-sufficiency of his very questionable process, can regret that the public should have the opportunity of hearing both sides of the matter. If the malcontent journals and individuals will have the kindness to point out where the analyses of Mr. Crookes, and Drs. Odling and Tidy are at fault, it will be quite time enough to demand that these reports should cease to appear, or that they should assume an aspect of "submissiveness."—I am, &c.,

W.

NOTES.

ACCORDING to Prof. Du Bois Reymond the electric organs of fishes afford a curious instance of organic adaptation to the water which they inhabit. The electric ray, or *Torpedo*, living in salt water, requires but a slight electromotive power, and its organ consequently resembles a battery of few elements, but of large section. The electrical eel (*Gymnotus electricus*) and the *Melapterurus* of the Nile require a great electromotive force; hence their organs are batteries of many elements, but of small section. A comparison of the conductivity of sea- and river-water shows almost the same relation as exists between the electromotive force of the organs of the *Gymnotus* and the *Torpedo*.

M. Musset ("Comptes Rendus"), after prolonged observation, has never seen a single insect captured by the leaves of *Drosera rotundifolia*.

Dr. T. W. N. Greene states that he practised for four years in a province of Monte Video, where the population, practically speaking, subsisted entirely upon meat, and yet scurvy was unknown.

The "Popular Science News" says—"It is the tendency of all modern training to take for granted that beasts—and men—are all alike; and, if they be not, to try and force them to behave as if they were."

S. R. Canestrini ("Rev. Scient. Indust.") has been experimenting on the effects of decapitation on insects. Butterflies are able to fly eighteen days after being beheaded; crickets leap on the thirteenth day; and *Mantis religiosa* was still able to move on the fourteenth. The house-fly has even been observed to copulate after decapitation.

M. Robin ("Les Mondes") holds that syphilis, and even the venom of serpents, may serve as prophylactics against the malignant fevers of hot climates.

A French writer ("Les Mondes") says—"The meteor (Darwinism) has cast a momentary lustre; it will soon disappear in the darkness of oblivion."

A statue of Le Blanc, inventor of the manufacture of soda, is about to be erected at Issoudun, his native place. When living the world refused this illustrious chemist bread; now he has been long dead she gives him a stone.

Cattle are maliciously destroyed in India by wounding them with a spike moulded from the seeds of *Abrus precatorius*. Death follows on the second day. The pounded seeds, taken internally, have little action.

MM. Muntz and Aubin ("Comptes Rendus") consider that we are living on a stock of combined nitrogen produced at "the beginning," and that we are exposed to see this stock decrease unless the supply produced by the action of atmospheric electricity is a sufficient compensation.

Dr. Laborde (Société de Biologie) considers cinchonamine as excessively poisonous, even in very small doses.

According to M. Poëhl (Russian Chemical Society) almost all the tissues of plants and animals have the power of converting albumenoid matters into peptones.

At the August meeting of the Entomological Society was read a communication from Dr. Fritz Müller, from which it appears that the protection derived by insects from an evil odour is not absolute. The author had found about thirty specimens of a common and very offensive Brazilian species whose wings appear to have been perforated by the beaks of birds.

The same author also finds that the pupæ of Lepidoptera are not invariably modified in colour in accordance with the object to which they are attached.

Dr. F. Müller also observed that the caterpillar of *Eunomia eagrus*, when becoming a pupa, attaches itself to a twig, and fixes its stiff and venomous hairs to the twig something in the style of a bottle-brush, so as to prevent access to the pupa either from above or below.

At the same meeting was read a communication from Mr. W. H. Pryor, on the insects of the Island of Yesso. About 11 per cent of the species collected there occur also in Britain.

M. A. Fauvel ("Comptes Rendus") still upholds the doctrine that the cholera has been imported into Egypt from India, in consequence of the suppression of preventive measures. It is significant that the Academy of Sciences has ordered the insertion of this paper in the "Comptes Rendus" in full, though exceeding the usual limits.

A. Ritter ("Wiedemann's Annalen") gives the following propositions as the results of a mathematical investigation of the subject:—The superficial temperatures of two fixed stars of equal densities are approximately as the square roots of their masses; the surface temperature of the sun was never much greater, and can never become much greater in the future than it is at present. The masses of the white fixed stars are greater than the mass of our sun.

A medical contemporary refers to the opinion that on the approach of cholera to a district sparrows and swallows forsake it in a body.

According to H. Victor Meyer the colour-reactions of benzol are due to the presence of a small quantity of a body hitherto unknown, which closely resembles benzol, and to which Prof. Meyer has given the name of thiopten.

We regret to announce that the museums and laboratories of the Indiana University have been destroyed by fire, caused by lightning. The loss is estimated at 166,000 dollars, but many of the specimens cannot be replaced at any cost.

"Science" considers it probable that the Biscayan fishermen visited Newfoundland before the days of Columbus. It appears that when Cabot landed he found the natives using the Biscayan word "baccalaos" for salt cod.

M. Gabriel Rogeron ("Les Mondes") recommends Bewick's swan for domestication, as being more beautiful than the common swan, and as taking up much less room.

M. C. Raveret-Watel, in the same journal, considers irrigation as the cause of a sweeping destruction of fish. The young fry penetrate into the channels, and perish when the current is suddenly shut off.

"Science" gives the following curious quotation from a book entitled "Eureka, or the Golden Door Ajar":—"If sound travels by vibration, as Science teaches, and Science teaches that vibration creates heat, that if a cricket should stand on one end of a solid slab-stone and rub his wings together, why is it that the vibration with the particles of stone does not completely melt the stone in ten minutes? I deny the hypothesis."

An "Entomological Congress," sparingly attended, was held at Paris on July 13th, in the Palais de l'Industrie.

After a turbulent discussion the French Chamber of Deputies has voted the proposed increase of M. Pasteur's pension to 25,000 francs annually, as a national reward.

According to M. Wroblewski ("Comptes Rendus") the critical pressure of oxygen is 50 atmospheres, and its critical temperature $-113°$ C.

M. de Chardonnet has shown that persons deprived of the crystalline lens, by the operation for cataract, perceive the ultra-violet rays more readily.

A specimen ("Science") of the American turret-spider (*Tarentula arenicola*) had been removed, together with its tower, the

upper and lower openings of which had been plugged with cotton to prevent the escape of the inmate. On the arrival of the package at its destination the upper plug was removed, but the lower one forgotten, The spider, therefore, began to cast it out, but during this operation she perceived that it would make a good material for lining the tower, and accordingly applied it to that purpose. This is a clear case of invention,—the utilisation of a novel material.

Mr. A. S. Packard ("American Naturalist") considers the Phyloptera (Neuroptera, Pseudo-Neuroptera, Orthoptera, and Dermaptera) as the nearest approach to the primitive groups from which insects are derived.

Herr A. Mehring ("Gesellschaft Natur. Freunde Berlin") proves, from palæontological evidence, that wild horses existed in Europe as far back as the end of the Tertiary period, and that the modern horse is not, as commonly supposed, of exclusively Asiatic origin.

The question put by Mr. W. A. Gibbs,—"Are we to lose our future harvests?"—now that a remedy has been found successful whenever tried, is again being emphasised by the doubtful weather of the present summer.

According to Baron Mueller palm trees reach their extreme southern limit in New Zealand, where *Kentia sapida*, a noble species, extends as far as latitude 44° S. The most southern American members of this tribe cease in La Plata in lat. 34°.

Mr. S. N. Rhoads ("American Naturalist") gives a positive proof that the turkey buzzard (*Vultur aura*) is guided to its prey by scent.

Mr. T. McIlwraith ("American Naturalist") gives a good account of the misconduct of the European sparrow, both as to the damage which it commits in the fields and gardens, and as to the too often successful warfare which it wages against harmless and useful species of birds.

Mr. D. H. Talbot, in the same journal, gives an interesting account of a mare fetching some men to rescue her colt, which was entangled among some willow trees on the bank of a river.

HH. Assmann, Von Bezold, and Van Bebber ("Naturforscher") reject the cosmic theory of the cold weather generally experienced from May 10th to 13th.

XUM

Gerald Massey's Lectures.

Previous to his departure for America Mr. Gerald Massey is about to give a Series of Four Lectures,—Archaic, Evolutionary, and Theosophic,—addressed to "thinking men and women," the first subject being "Man in search of his Soul during 50,000 Years."

This Course of Lectures is advertised to be given on the last four Sunday afternoons of September, at 3 p.m., in St. George's Hall, Langham Place.

[The above Lectures do not come within our cognizance as far as their principal subjects are concerned. They will, however, we understand, discuss incidentally the origin of language and the transit from the brute to the human condition.—Ed.]

Erratum.—In the article by O. Reichenbach in the August number, page 456, 4th line from top, $\frac{M}{D^2}$ should be $\frac{M}{D^\infty}$.

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